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Journal
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Vol. 239 JANUARY, 1945 No. 1

CERTAIN APPLICATIONS OF PHYSICAL PRINCIPLES TO THE PLAYING OF MUSICAL INSTRUMENTS.*

BY

W. F. G. SWANN,

Director, Bartol Research Foundation of The Franklin Institute, Swarthmore, Pennsylvania.

Introduction. As a result of the perusal of various forms which I have at the Bartol Research Foundation, I have come to the conclusion that if, on this occasion, I should perform my annual duty of reporting to you upon the activities of the Foundation, I should be fined $10,000 and be put in jail. Not relishing either of these prospects, I have meditated upon the question of what other matters I could talk about this evening which would be safe, worth while, and possibly interesting.

I have spoken so frequently upon cosmic rays that you would not, I know, wish to hear more of that. I might turn prophetic and tell you what science is going to do after the war, but then, unless I should depart from this sphere in the meantime, I should probably have to eat my own words in the future.

For all of these reasons, I have decided upon a subject which cannot harm anybody but the artists, and while some of them may become very angry with me, I am confident in the hope that they may not be able to do very much about it. And so I propose to talk about one or two matters which have been of interest to me as a physicist and in connection with my avocational activities in music. I wish to say at the outset that I shall attempt no systematic treatment of the relation of physics to music; that is far too large a subject for one evening. What I have to say will concern a rather disjointed set of things which are chosen with

* Presented at the Stated Meeting held on March 15, 1944. The text of this paper will be presented in semi-popular vein. However, the paper will be supplemented by more technical footnotes and by an appendix, which will appear at the end of the third section of the paper, and to which reference will be made in the text.

(Note—The Franklin Institute is not responsible for the statements and opinions advanced by contributors in the JOURNAL.)

little regard to their interrelationship, and simply because I believe them to be matters which have a worthwhile significance to the artist.

It has been my general experience that whenever there is a difference of opinion between a musician and a physicist concerning some point having to do, primarily, with the musical result attained, the musician is, in 90 per cent. of the cases, right and the man of science is wrong. However, matters do not end here, because the musician usually makes the mistake of giving the scientific reasons for what he says. Then everything that he says is wrong, and the man of science has only to stand by and jump on him at the end of every sentence, reducing him to a state of complete defeat, but leaving him unconvinced, nevertheless.

Realizing that many members of the audience are not technically trained to any large degree in science, I shall speak in the most elementary terms. I know that my colleagues, the physicists, will sympathize with me and forgive my dwelling upon certain matters which, to them, may be very obvious. What I have to say will constitute rather a cafeteria fare without any special continuity which would make the lecture an ordered section of a logical whole. I intend, in fact, as I have already said, to concentrate upon a few isolated matters which I have chosen simply for their general interest, because, moreover, any extended treatment of the subject would be impossible in the space of an hour and because, with the pressure of other matters at the Bartol Foundation, I have not had the time to do any very extensive work as part of a more complete study of the subject.

STEEL VERSUS GUT STRINGS.

I will first speak of a matter which has become of increasing interest in recent years and having to do with the use of steel strings in place of gut upon stringed instruments such as the 'cello and the violin. I have found a good deal of confusion of thought among artists and others as to the significance of what is implied. I have encountered people who are fearful that the use of steel strings will result in some terrible pressure upon the structure of their instrument which will ultimately bring disaster.

Now the fact is that it is perfectly possible to choose the diameter of the steel string in relation to the gut string in such a manner that, when one replaces the other, the stresses on the instrument will be exactly the same when the two strings are tuned to the same note. The starting point of the theory in this matter is very simple. We all know that a definite note is determined by a definite number of vibrations per second of the string which produces the note. It is easy to show from acoustical theories that if n is the number of vibrations per second which a string makes in emitting its note, if l is the length of the string between bridge and nut, if T is the total tension on the string, and m the mass per unit

length of the string, then

$$n = \frac{1}{2l}\sqrt{\frac{T}{m}}.$$

It will be observed that if two strings have the same length and the same tension T, they will emit the same note, provided that the mass per unit length of each string is the same or, in other words, provided that the total masses of the strings are the same (since their lengths are equal).

Of course, the force on the body of the instrument will be determined not only by the tension T, but also by the angle θ between the bridge and the string.[1] If this angle were 90°, so that the string were straight from tail piece to nut, there would, of course, be no pressure on the belly of the instrument.

Now it is easy to see what we must do in order to make the mass per unit length the same for each string. If subscript s refers to steel and g to gut, we must have

$$\pi r_s^2 \rho_s = \pi r_g^2 \rho_g,$$

where ρ_s and ρ_g are the densities of steel and gut respectively. It follows from this that

$$\frac{r_s}{r_g} = \sqrt{\frac{\rho_g}{\rho_s}}.$$

Since the density of steel is five times that of gut,[2] we see that r_g/r_s must be equal to the square root of 5, which is 2.24. If we make the radius of the steel string smaller than that of the gut string in this proportion, the two strings will give the same note when they have the same length and are pulled with the same force.

I have here an experiment to illustrate this matter.[3] Two strings, a gut and a steel, are stretched on this board, each with a weight of 20 lbs. hung from its end. The length is determined as the distance between two bridges and is the same for both. It is 37 cms. Each string, when plucked, gives, as you hear, a note which is approximately the same. Now we will project these strings upon the screen in such a way that they are both magnified to the same extent, and it will be seen that the diameter of one is a little more than twice that of the other.

Of course, the strings used in practice are not entirely steel. In all cases except that of the steel violin E, the steel forms only a core, which is wound with other wire, as are the ordinary lower gut strings of the 'cello and the G string of the violin. However, this does not matter.

[1] In general, this angle may be different on the two sides of the bridge. However, this introduces no elements which invite consideration in relation to what is here discussed.

[2] As we shall presently have occasion to remark, the mass per unit length of a gut string varies considerably with the humidity.

[3] The experiment is illustrated by Fig. 1 with its caption.

All physics cares about is the mass per unit length of the string, and as regards other things, it does not care what the string is made of.

It may be of interest to see how the situation as regards choice of strings has worked out in practice. A noted violin maker has kindly lent me a set of steel strings and gut strings for the violin and 'cello. I have weighed them and calculated the ratio of tension steel to gut for each pair of strings when the two strings concerned are tuned to give the same note. The results are shown in Table I, in which the column headed "Ratio" gives the ratio of the tension for steel to that for gut.

TABLE I.

Origin of Strings	String	Ratio
Skilled	Violin E	1.05
Musical	Violin A	1.44
Instrument	Violin D	1.39
Maker	Violin G	1.53
Skilled	Cello A	2.03
Musical	Cello D	1.83
Instrument	Cello G	1.62
Maker	Cello C	1.39
	Violin E	0.709
Professional	Violin A	0.826
Violinist	—	—
	Violin G	1.64

In the same Table, I have shown the results for another set of strings with which a well-known local violinist supplied me as representative of the particular strings he was using.

It will be observed that in the case of the strings in the columns labeled "Professional Violinist" there was actually less tension on the E and A strings when steel was used than there was with gut. In the case of the G string the condition is reversed. In the case of the other set of violin strings and in the case of the 'cello strings, there is, in general, much more tension for the steel strings than for the corresponding gut strings. For the 'cello A, the steel string requires twice the tension of the corresponding gut string. However, in the case of another steel A of the type known as tomastic, the ratio is considerably less than 2. It is possible that more attention has been given to the scientific aspects of the problem in the case of metal strings originally put out than has been given by later manufacturers.

There is one peculiar thing which must be mentioned in connection with this matter. It is something which I did not know when I started to prepare this lecture and which forced itself upon me.[4] It concerns

[4] The fact that the weight of a gut string can vary considerably by taking up or giving out water is apparently well known. While the influence of this circumstance upon string tension is obvious, its significance is not, I think, realized in its full importance.

gut strings. Everybody is familiar with the fact that when you put a new string on a 'cello or violin it goes down in pitch for some time, but that is usually attributed to inelastic stretching and consequent release of tension. Subsequent changes in pitch are usually attributed to the same cause. However, in the apparatus which we have used for demonstration (Fig. 1), there was no chance for the tension to alter, because it

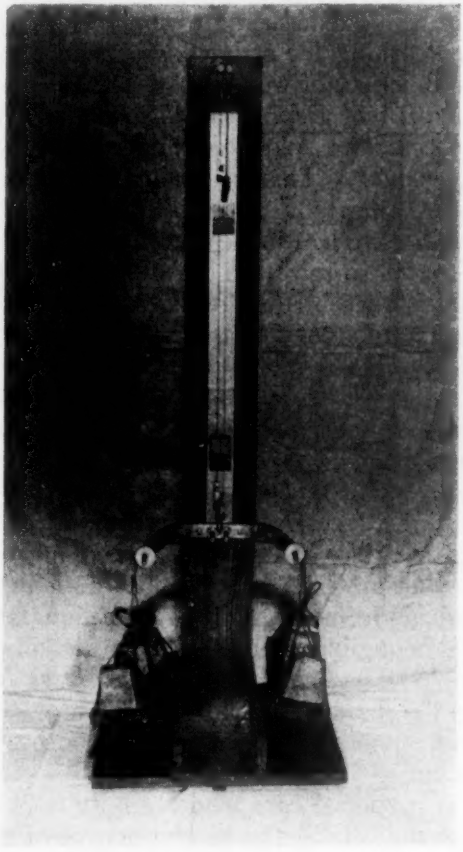

FIG. 1. The elements of the apparatus are shown in the figure. The two equal weights are seen at the sides. They maintain upon their respective strings tensions which are equal and independent of any stretching of the strings.

The optical system for demonstrating the apparatus is not shown.

was determined by a fixed weight. Yet the pitch of the string altered considerably with time, and not in a very consistent manner. There was only one possibility; the mass of the string must be changing; and the only way in which that could change would be by the string taking up or giving out water vapor. I consequently took a string and put it in a closed chamber all night in company with a beaker of water, so that

the air around it became humid. I then weighed it. Its weight was 4.03 grams. I then put it in a desiccator with a drying agent and left it all night. Its mass became 2.93 grams. In other words, the string which had been in the moist atmosphere was 37 per cent. greater in weight than the string which had been in the dry atmosphere. This means that the moist string would take 37 per cent. more tension to make it give the same note as was given by the same string when dry. If the tension were held fixed, the note would fall by nearly a tone and a half. This is something for string instrumentalists to ponder over when considering the effect of weather upon their instruments.

Effect on Intonation of Depressing the Finger. When playing the instrument, it is necessary to place the finger upon the string and press it down to the fingerboard. This results in a stretching of the string and so in an alteration of the frequency of vibrations of the note from the value which it would have had if the fingerboard had been very close to the string. One may also raise the question of whether, in depressing the string a given distance h to meet the fingerboard, the finger has to exert a force which is different for steel and gut. It turns out that the percentage alteration in frequency of vibration and the percentage alteration in tension are of the same order of magnitude, the alterations being about ten times as large for steel as for gut, although small in each case. Now even a small percentage alteration in frequency is of importance. However, for a given extent of depression of the finger at a certain place on the instrument, the force which the finger must exert depends only upon the ultimate tension of the string. The main part of this tension is the tension which exists before the string is depressed, and the alteration resulting from the depression, although larger for the steel than for the gut, is negligible in either case as far as practical considerations are concerned. The finger pressure necessary for a given depression h will thus be the same for steel as for gut, provided that the strings are chosen to "correspond" in the sense that both give the same note for the same tension.

Let us consider a special case where a note is being played on the 'cello A string, the note being the second octave of the open string, so that the finger is applied one quarter of the way from the bridge to the nut. Let the finger depression h have the rather large value 1 cm. Then it turns out that [5] the frequency will be altered by about 7 per

[5] From (11 A) and (12 A) in Appendix (Problem 1) we find that if δT is the alteration in tension and δn the alteration in frequency, then to a sufficient approximation

$$\frac{\delta n}{n} = \frac{h^2}{16\,ab^2n^2}\left(\frac{Y}{\rho}\right); \qquad \frac{\delta T}{T} = \frac{h^2}{8\,ab^2n^2}\left(\frac{Y}{\rho}\right),$$

where h is the finger depression, Y is Young's modulus of elasticity, ρ the density, n the frequency of the note, $(a + b)$ the length of the string, and b the distance from the bridge to the finger. Inserting $Y = 2 \times 10^{12}$ dynes/cm.^{2} for steel, $\rho = 7.5$, $(a + b) = 70$ cms., $(a/a + b) = 1/4$, $n = 880$, we find $\delta n/n = 0.07$, and $\delta T/T = 0.14$. Both quantities are proportional to Y/ρ, which is ten times as large for steel as for gut.

cent., that is, about half a tone for a steel string, as compared with one tenth of this amount for a gut string. Although the alteration in tension resulting from depression is ten times as large for steel as for gut, it amounts to only 14 per cent. in the former case, and such a change in finger pressure would be unobservable.

It must again be emphasized that the conclusion that the finger pressure for a given finger depression is practically the same for steel as for gut holds only if the strings compared "correspond" in the sense of having the same mass per unit length. Thus, it would not hold for the strings compared in Table I. In fact, the steel 'cello A there cited would require twice the finger pressure required for the gut string. Since the finger pressure is proportional to T and to h,[6] it will be necessary to have only half the distance between string and fingerboard if we are to have the same finger pressure for the steel and gut 'cello A strings cited in Table I.

MATTERS PERTAINING TO THE PRESSURE ON THE BOW IN STRINGED INSTRUMENTS.

There is a good deal of misunderstanding in relation to the amount of pressure on the bow which is needed when a stringed instrument is being played. In the hands of the artist, these matters have as a rule shaken themselves down into a correct procedure in which the artist, functioning as a teacher, can produce the desired results himself. He is apt to have a pretty explicit notion of what he is doing, a rather less explicit picture of the mechanical principles involved, but a combination of the two which is sufficiently complete to lambaste the unfortunate pupil over a sufficiently large number of years into doing likewise. There remain from all these things, however, certain matters which are imperfectly understood by some artists, although they may be understood by others, and there arise in connection with them rather vague admonitions against forcing the tone, and so forth.

[6] If T is the tension of the string with the finger depressed, the force F exerted by the finger is given by

$$F = F_1 + F_2,$$

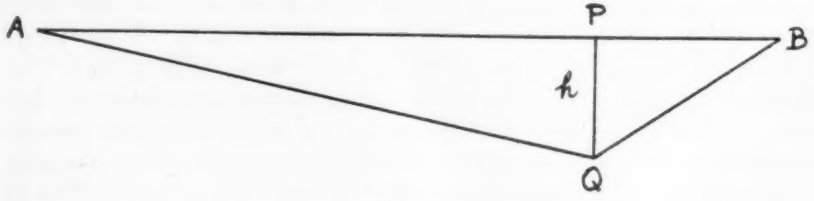

FIG. 2.

where

$$\frac{F_1}{T} = \frac{h}{QB}; \qquad \frac{F_2}{T} = \frac{h}{QA},$$

$$F = hT\left(\frac{1}{QB} + \frac{1}{QA}\right) = hT\left(\frac{1}{PA} + \frac{1}{PB}\right) \text{ approximately.}$$

A good many people do not realize that the volume of tone obtainable from a stringed instrument, when played at an assigned distance from the bridge, does not depend appreciably upon the pressure of the bow, but only upon its velocity.[7] The production of great pressure, however, more or less mesmerizes the player into the belief that he is producing big tone, but alas, it is only he who hears it. Then, while practically everybody recognizes that when the bow is at the point greater forces have to be exerted by the fingers holding it than are exerted when it is at the heel, I doubt whether many realize the extent of this difference.

Figure 3 symbolizes a bow pressing upon a string at a certain point. At this point, there is a force P which is upwards on the bow and corresponds to an equal and opposite force on the string. It is this force P which it is the object of the player to keep constant for all positions of the bow, unless, for some reason or other, he has a definite intention to vary it. The point is that if he varies this force, it should be because he intends to do so and not because he is unable to do otherwise or is unaware of the fact that he is varying it.

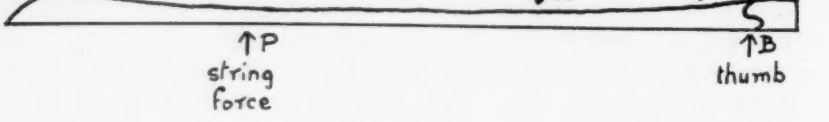

FIG. 3.

In the Appendix (Problem 2) to this article will be found a detailed analysis of the essentials as regards the forces to be applied by the fingers [8] for the various points of contact of bow and strings. Some of the conclusions there reached will be obvious, at least qualitatively, at first glance, while others are not quite so obvious. In the first place, of course, since the strings of a violoncello, for example, are inclined to the horizontal, forces must be exerted to prevent the bow from slipping down the strings. However, the forces necessary on this account present no particular difficulty in our understanding of the problem. Indeed, they would be always the same, regardless of the point of contact of bow and string, were it not for friction which participates in preventing the slipping of the bow down the strings to an extent which calls upon the fingers to supply forces which vary with the point of contact of bow and string. The forces concerned in this matter are readily taken care of in a subconscious manner, and the subconscious actions are not

[7] See, for example, F. A. Saunders, *J. Acous. Soc. Am.*, **9**, 81–98 (1937); also R. B. Abbott, *J. Acous. Soc. Am.*, **7**, 111 (1935).

[8] In using the word "fingers" in the collective sense I, of course, include the thumb as a finger.

materially strengthened by a mechanical analysis of the problem as, I believe, are the actions concerned with the more vital finger forces necessary to supply the force P with which the bow presses on the string in a direction perpendicular to the length thereof. In what follows, therefore, I shall concentrate entirely upon the components of the forces which are perpendicular to the string. Thus, for example, the weight of the bow acts vertically downwards, but it may be regarded as having a component which acts perpendicular to the string. We shall speak of this component, W, as the resolved weight of the bow.

Now it turns out that the essential forces which are concerned in our analysis are: (*a*) the force P which we desire to create as acting on the string at the point of contact, and therefore, as acting from the string, in the inverse sense, on the bow at this point; (*b*) a force A, produced by the first finger, and acting downwards—understood as perpendicular to the string; (*c*) a force B, produced by the thumb, and acting upwards—in the sense perpendicular to the string; (*d*) a force C exerted by the little finger, and acting downwards—in the sense perpendicular to the string; (*e*) a force W, representing the resolved weight of the bow acting through the center of gravity of the bow. It will turn out that, of the three forces, (*b*), (*c*), (*d*), only two need be operative for any position of the bow on the string, the particular two depending upon that position.

It will be convenient to consider first the ideal case where the weight of the bow may be neglected. In the particular case where the contact of bow and string is immediately under the first finger, the thumb, of course, exerts no force; and to put myself in harmony with the starting concept voiced by most instrumental players, I will say that under these conditions the downward force transmitted through the first finger is caused by the weight of the arm, where I use this expression to imply, not necessarily the whole weight, but that portion of its contribution which is left over after as much as is deemed necessary is removed by muscular support of the arm at the shoulder.[9]

[9] We can, in fact, think of the arm as a pendulum pivotted at the shoulder. Regardless of the "convolutions" of the arm or wrist, the force applied by the bow to the string in the conditions cited in the text will be applied in a direction perpendicular to the plane which contains the axis of rotation at the shoulder joint and the point of contact of bow and string. It is, therefore, necessary that the aforesaid axis should pass through the string which is being played, as otherwise the force arising from the weight of the arm will not be perpendicular to the string. The shoulder axis of rotation is, of course, adjustable in direction. Its normal position approximately satisfies the conditions specified. Incidentally, the force between bow and string must not be merely perpendicular to the string but must, in addition, be perpendicular to the fingerboard in a plane at right angles to the string, so that the shoulder axis, in addition to passing through the string, must be parallel to the plane of the fingerboard below the string which is being played. In the foregoing requirements we see the exemplification of a principle, sensed by many 'cellists, to the effect that the body should experience a rotation from the position appropriate to playing on the A string to that appropriate to playing on the C string. Viewed from the mechanical standpoint, this rotation is merely a means of securing the shoulder axis conditions above cited.

Now starting with the weightless bow on the string at a point just below the first finger, as we draw it so that the point of contact with the string moves towards the point of the bow, the thumb has to come into play and exert an upward force B, as otherwise the bow would fall. We must also increase the force due to the first finger by an equal amount, so that $A - B$ is always equal to the force P which we wish to exert on the string. As we move the bow farther and farther along, the equal and opposite additions by the thumb and first finger increase continually. The equal and opposite forces contributed by the thumb and first finger (over and above the force P also contributed by the first finger) constitute what the physicist calls a couple—in simple language, a pure twist. It is easy to see that the magnitude of that twist will have to be greater the smaller the distance between the thumb and the first finger. If the thumb were right behind the first finger, one could not, by normal processes, exert any twist at all, and the only process by which the pressure on the string could be maintained in the condition of Fig. 3 would be the very bad process in which, by pinching the bow sufficiently tightly, one could prevent everything collapsing. Indeed, there would, even in this case, arise a twist, but it would be a twist produced by ill-defined methods foreign to a clear-cut muscular action such as becomes possible when a finite distance between finger and thumb is maintained. I point this out, because, while everybody realizes that there should be such a distance, I doubt whether all students of stringed instruments realize the important part played by that distance. Considering a 'cello bow, it may be of interest to remark that in order to produce a certain force P when the contact of bow and string is at the point of the bow, we have to exert with the first finger a force 15 times P, and an upward force 14 times P when the distance between thumb and first finger is 4 cms. and when, as stated, we neglect the weight of the bow.

While the weight of the bow does not affect the gross features concerned in the above discussion, it does play an important part in other matters, particularly in those cases where the point of contact of bow and string is near the frog. These features are discussed, as already stated, in the Appendix (Problem 2), and, in particular, illustrative graphs are drawn for two cases, namely, where the desired force P is 100 grams (0.22 lb.), and where it is 200 grams (0.44 lb.). It will be sufficient here to summarize the results for the following typical case:

Weight of bow	75 grams (0.165 lb.)
Distance from frog to center of gravity of bow	17 centimeters (0.62 ft.)
Desired force to be exerted by bow on string	200 grams (0.44 lb.)
Distance between thumb and first finger	4 centimeters (1.6 inches)
Distance between thumb and little finger	4 centimeters (1.6 inches)

Starting with the bow at the "point," it is necessary for the first finger to exert a downward force of 3,000 grams (6.6 lbs.) and for the

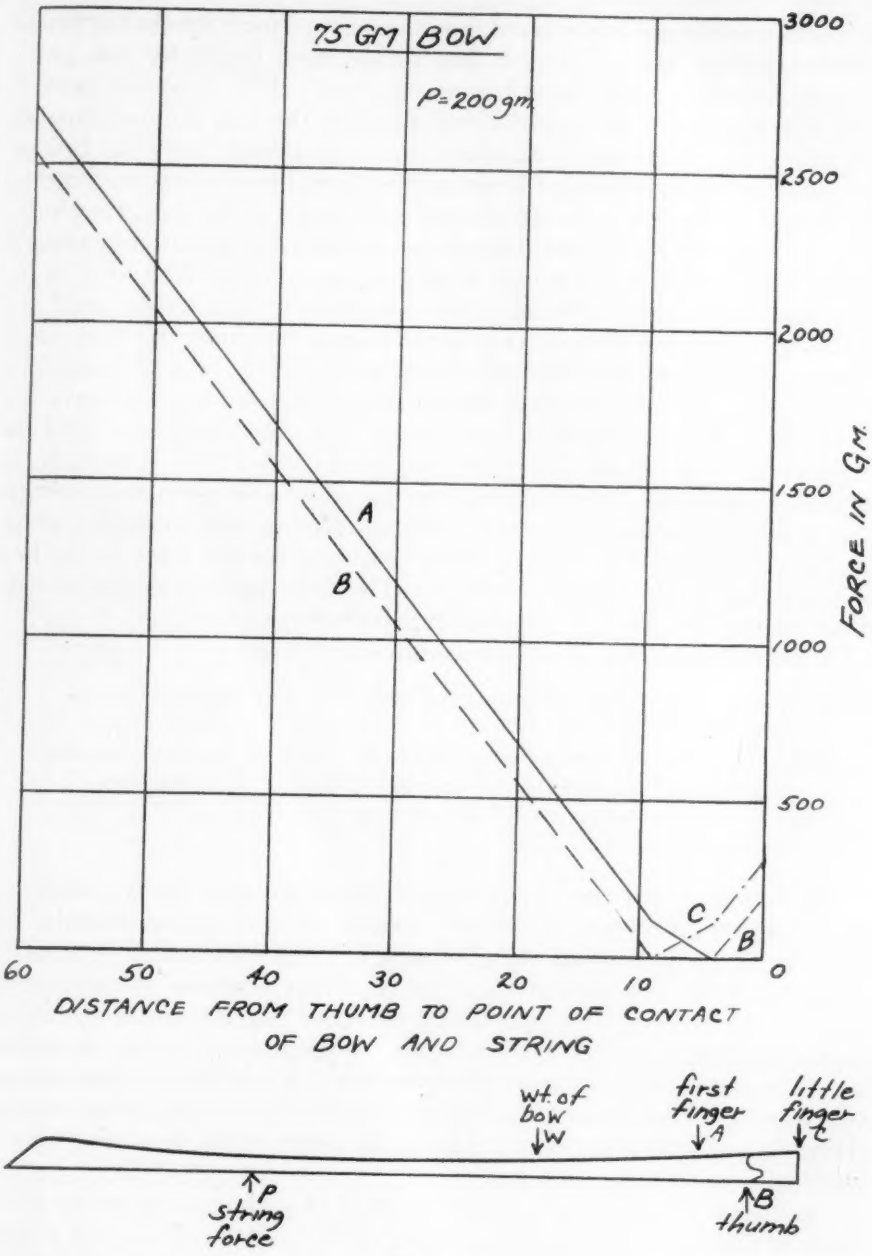

FIG. 4.

thumb to exert an upward force of (3,000–200) grams ((6.6–0.44) lbs.). The little finger is completely inoperative at this stage. As we move the bow towards the frog, the force exerted by the first finger diminishes in

proportion to the amount moved, but the upward force due to the thumb remains always less than that due to the first finger by 200 grams (0.44 lb.), and the little finger has no function. This condition prevails until the distance of the point of contact from the frog is approximately 9 centimeters, (3.5 inches), at which point the thumb force has become zero, and only the first finger is operative, the balance being produced by the weight of the bow. As we proceed still nearer to the frog, the thumb remains inoperative but the little finger comes into operation in amount proportional to the further bow displacement, and the force due to the first finger continues to diminish, but less rapidly than before, until at a distance of 3.9 centimeters (1.5 inches) from the frog, the first finger exerts no force, and only the little finger is operative. As we move from this point to the frog, the first finger remains inoperative, the force due to the little finger continues to increase at an increased rate, and the thumb once more comes into play, the force caused by it continuing to increase regularly. These conditions are shown in graphical form in Fig. 4, which is self-explanatory, and Fig. 5 is the corresponding graph for the case where the constant value required for the force of the bow on the string is 100 grams (0.22 lb.). The calculations associated with these curves are given in the Appendix (Problem 2).

To summarize the situation, we have as follows:

From point to distance 9 centimeters (3.5 inches) from the frog	First finger and thumb alone operative
From distance 9 centimeters (3.5 inches) from frog to point 3.9 centimeters (1.5 inches) from frog	First finger and little finger alone operative
From distance 3.9 centimeters (1.5 inches) from frog to frog itself	Thumb and little finger alone operative

Of course, upon the above-named forces we can always *superpose* three additional forces, A', B', C', caused by first finger, thumb, and little finger, provided that they balance, in the sense that $A' + C'$ is the negative of B', and provided further that they combine to produce no twist.[10] However, the forces which we have discussed and which are represented in the graph constitute the simplest set of forces necessary.

I have here a simple apparatus by which it is possible to illustrate the variations of thumb and finger forces necessary to secure a fixed bow-to-string force at various distances from the point of the bow, and we will perform certain tests with it.[11]

In another experiment, I have a wheel placed approximately at the point of contact of a bow with a string in the case of one playing a 'cello seated upon this chair. The gauge, whose readings are recorded on the

[10] The condition for this is $A'a = C'b$, where a and b are respectively the distance of the first finger and little finger from the thumb.

[11] At this point the apparatus shown in Fig. 6 was demonstrated. The nature of the apparatus and of the demonstration will be sufficiently obvious from the caption.

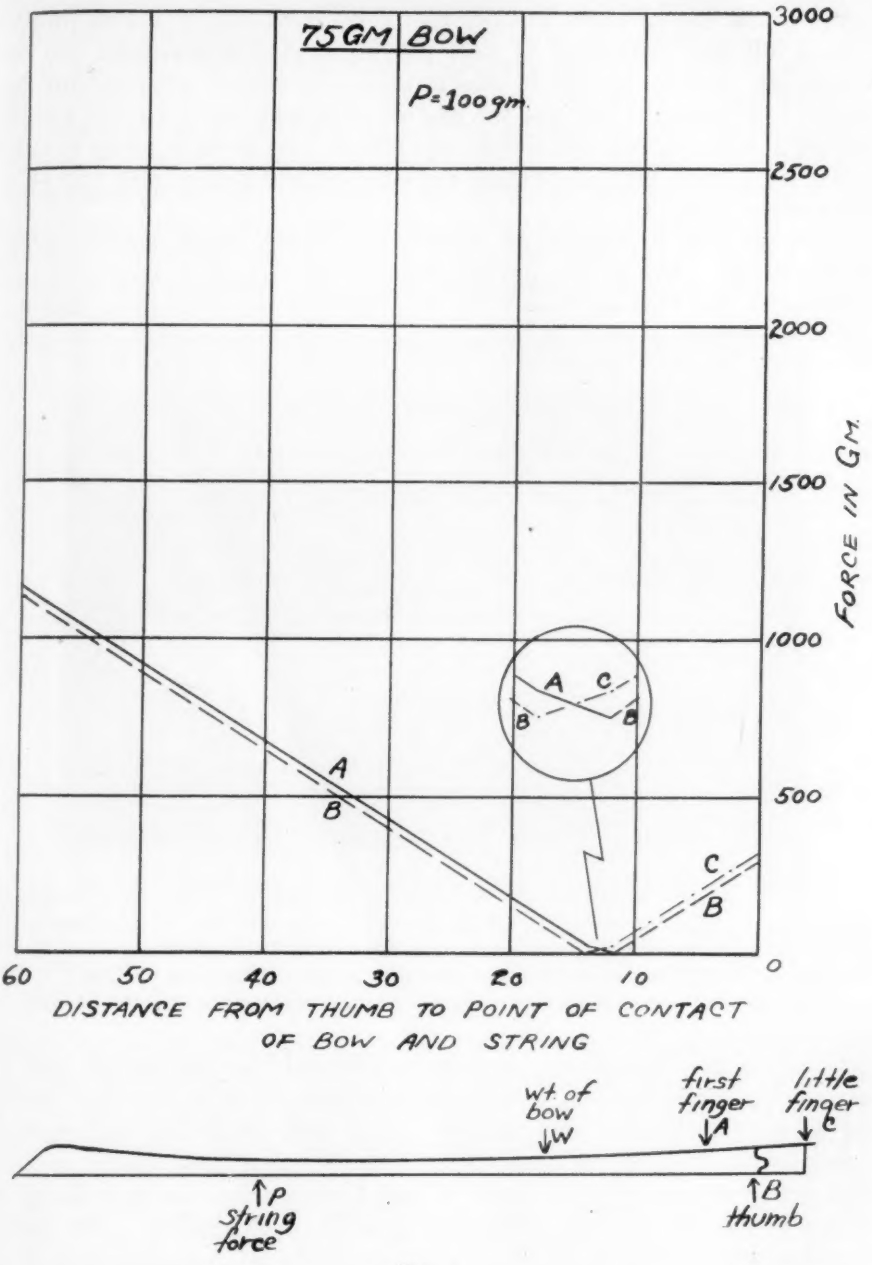

FIG. 5.

screen, represents the force which a player would be exerting on the string if he played under these conditions. Now I have in the audience a number of amiable guinea pigs who are willing to coöperate, and I am

going to ask one of them to take the 'cello bow, put it in this pulley, draw a full bow as though he were playing on the instrument, and see how much trouble he has in maintaining a constant pressure on the pulley. Or rather, I will put the matter in another way. I will not let him see the gauge, but will simply ask him to bow in such a way that he thinks he is producing a constant pressure on the pulley, and you shall observe the result.[12]

Effect of Bow Pressure upon Intensity of Sound, Together with Certain Preliminaries. My next experiment has to do with the use of an actual instrument and is concerned with showing how the volume of tone depends upon bow velocity and with showing further that it is largely inde-

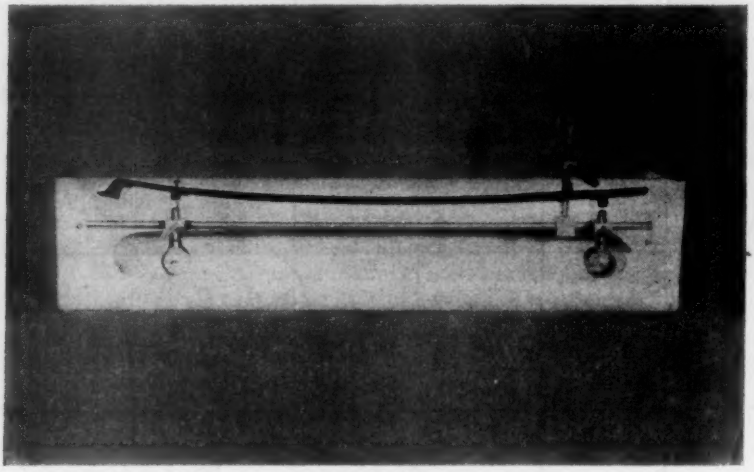

FIG. 6. The three gauges measure the forces corresponding to P (the force of the bow on the string), A (the force due to the first finger) and B (the force due to the thumb). By varying the point of application of P, it was possible to trace the variations in A and B over the range in which C was inoperative, and show that $A-B$ is constant and equal to P.

The apparatus was supplemented by optical arrangements for projection on a screen.

pendent of bow pressure. In this experiment the instrument will be played into a microphone, and the microphone will record its indications on what is called an oscillograph. This very beautiful oscillograph, specially designed for demonstration purposes, has been kindly lent for this demonstration by the RCA Victor Corporation through the good offices of Dr. Vladimir K. Zworykin and Mr. T. A. Smith. Mr. Smith has also been so kind as to arrange for one of the RCA engineers to come here this evening and set up the instrument. I wish to take this oppor-

[12] The apparatus is shown in Fig. 7 which is sufficiently explained in its caption. Three people who played the 'cello operated the device. In only one case was anything like a constant bow pressure maintained throughout the length of the bow.

tunity of expressing my sincere appreciation to the RCA Corporation and to Dr. Zworykin and Mr. Smith for these courtesies.

Now, first I must tell some of you what an oscillograph is, and as a prelude to that, I will again remind you that sound is produced by vibrations of the sounding body which transmit themselves through the air and set the ear-drum, or microphone, as the case may be, into corresponding oscillations. In the present case, the microphone is connected to this instrument, the oscillograph, in such a way that as its diaphragm vibrates backwards and forwards this green spot moves up and down. The microphone is now receiving a sound and you observe that the green

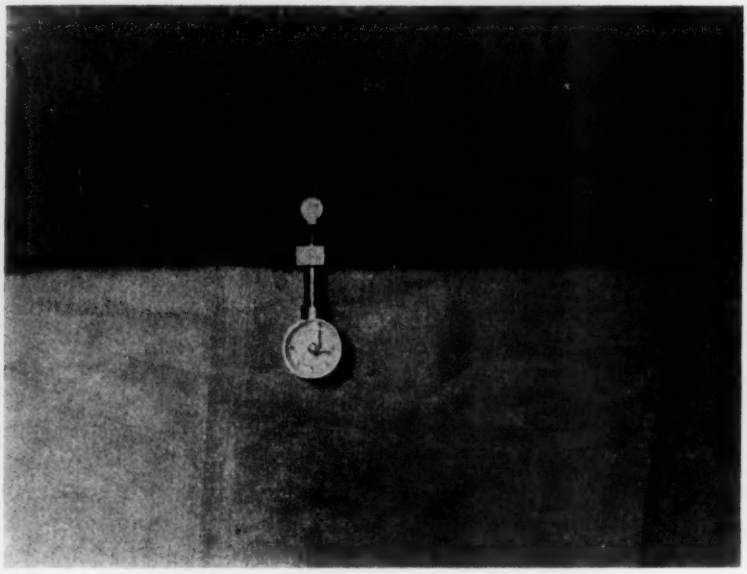

FIG. 7. The gauge was mounted on a firm pillar (not shown) so that it was inclined at an appropriate angle, and its indications were projected upon a screen by an optical system not shown in the figure.

spot is drawn out into a vertical line. However, at this stage you cannot see very much regarding the nature of the vibrations. To illustrate the next point, I call your attention to an analogous situation. I have in my hand a lamp which I can cause to oscillate up and down with a certain rapidity, a certain frequency of vibration, as we say.[13] On account of the persistence of your vision, you observe simply a vertical streak of light. If, however, while all of this is going on, I move my arm sideways, that oscillating, vertical streak of light is drawn out into a wavy curve for a reason which I think is sufficiently clear to render unnecessary any further explanation. It will suffice to say that the curve

[13] This apparatus, shown in Fig. 8, is sufficiently explained by its caption.

appears to be more or less continuous because, on account of the persistence of your vision, you do not see the lamp only at one place at a time, but you see it simultaneously at all places which it has occupied during a period of a tenth of a second or so.

Now, turning to the oscillograph, if, while the spot is oscillating up and down as it is doing now, I cause it to move sideways, its story will be drawn out into a wavy curve. It will be immediately obvious that the closeness of the wiggles in this curve will be greater the higher the

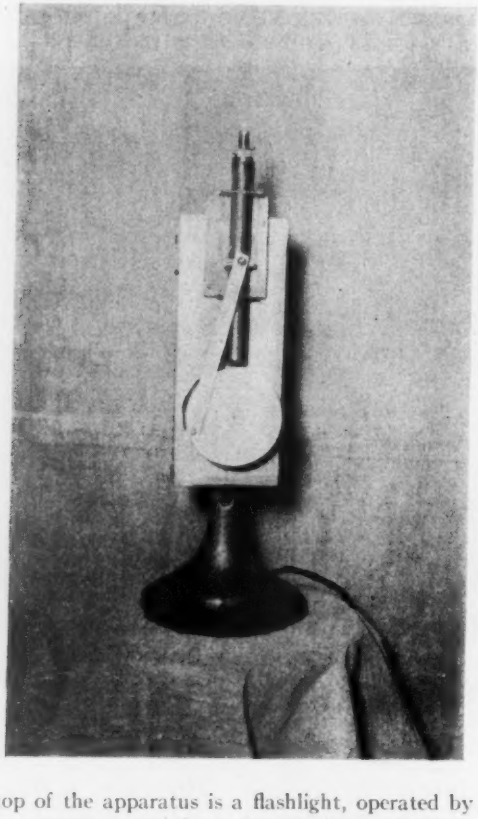

FIG. 8. At the top of the apparatus is a flashlight, operated by a small dry battery. The light can be made to move up and down sinusoidally by rotation of the wheel, which is brought about by the fan motor at the back.

frequency of vibration. In fact, the number of wiggles per foot will be proportional to that frequency of vibration.

I must just add one further remark. In the present instrument the horizontal motion occurs from left to right in a time equal to an integral number of cycles of the whole vibration, then it sweeps back in practically infinitesimal time and repeats the motion from left to right so that the next cycle of motion, with all of its characteristics, is imposed upon the first, and so on.

The curve at present shown on the oscillograph represents a pure tone, corresponding in fact to about the middle C of the piano. I will superpose upon it a fifth,[14] and you will see that we get a more complicated pattern. By superposing upon it higher overtones, as they are called, we can get quite a complicated pattern. In fact, any complicated pattern of this kind can be resolved into a simple, fundamental tone, and a lot of other higher tones—overtones. The mathematician knows well how to do this. Now, as a rule it is a fundamental, the lowest tone, which characterizes to the ear what is recognized as the pitch, and the ear loses consciousness of the other tones for the most part as individual entities and incorporates them into a conglomerate sensuous impression which we speak of as the quality of the note. It is in fact these overtones which *determine* what we call the quality.

I will again play into the microphone the note C from a recorder. This note has very little but its fundamental. I will play the same note on the 'cello. You will see that the pattern is much more complex. With your still greater indulgence, I will sing the note. I will sing it again, altering the quality of my voice. Finally, I will play this note upon the piano. Here, incidentally, you will note a rather peculiar phenomenon. As the note dies down in intensity—in loudness—its quality changes. We thus have to recognize that on the piano at any rate, and to some extent on other instruments, the quality varies during the period in which the note is being sounded.

I will take another illustration by playing a single note upon two different 'cellos, and I will play the same note in different places on the same 'cello. You will observe that the character of the note is quite different, depending upon which string and part of the string is used. I will play the same note twice on the same place on the 'cello, but with different positions of the bow on the string. Again, the character is quite different. Of course, all of these characteristics are more or less observable by the ear, but since the ear so often hears that which it expects to hear or wishes to hear, or which its master, the brain, so closely watching over it, tells it it ought to hear, it is more convenient for this lecture to rely upon the more impartial microphone which has no soul and which, even though it may lie, lies consistently.

As I have already implied, the vertical extent of the motion on the oscillograph gives us a measure of the loudness of the tone.[15] I will consequently use the oscillograph to illustrate one or two of the matters to which I have referred in relation to bow pressure. I will play a note in a certain position on the string by bowing with a certain bow speed and pressure. I will now play that same note with the same bow speed but with a greater pressure. You will observe that there is very little

[14] The note and its fifth were played upon two recorders.

[15] I am not here concerned with absolute comparisons of intensity, so I refrain from referring to the logarithmic relation between acoustical energy and perceptual intensity.

alteration in loudness. I will increase the pressure to the point of scratching, and while the quality changes, there is no great change in loudness. I will diminish the pressure, using the same speed. Again there is very little change in loudness. Thus over a wide range of bow pressure, we get the same intensity. If, however, I diminish the bow pressure beyond a certain limit with the same bow speed, not only do I obtain a smaller intensity, but the quality of the note becomes more simple and orally I get the impression of what is commonly referred to as a thin tone. This thinness is probably due more to alteration in quality than to alteration in intensity.

If, now, I play the note again with a normal pressure and then increase the velocity of the bow, you will immediately observe a much larger intensity. And even if I press quite lightly, I obtain this large intensity.

Now, since it is not immediately obvious, or at any rate satisfactory to you, to believe that I am varying the pressure in the way I have indicated, I will repeat some of these experiments with a mechanical method of drawing the bow and realizing the pressure. This robot has not studied the playing of the 'cello for very long, so that its sounds are not as pleasing as they might be. However, you will observe that I have here [16] a wooden rod with a pulley at the end, and the pulley can rest on the top of the bow. By placing weights on this stick, I can alter the force on the bow. I will tie a string to the end of the bow, so that by means of a motor I can pull the bow along with constant velocity. I will simply use my hand to guide it.[17]

It is very fortunate that there is this independence of intensity upon bow pressure, because if such were not the case the player would have a much harder task to secure a uniform intensity throughout the range of the bow.

Incidentally, I may remark that by watching the oscillograph one can have some visual guide as to the constancy of his tone quality and intensity, and it is possible to observe, even in the playing of a passage, faults arising from false accents, and the like. Of course, such things are immediately observable to the trained ear, but it is sometimes difficult to persuade the not so well-trained player that he is committing sins greater than he believes himself to be committing. Thus, frequently, in leading up to a high spot, particularly at the point of change of bow, the player will diminish his bow velocity so that there will be a kind of hole in the tone just at the place where he desires the greatest continuity of sound. I will illustrate this matter.[17a] You will observe that just prior

[16] The apparatus is shown in Fig. 9. The combination of the caption with the description in the text will provide an understanding of its operation.

[17] Here followed some demonstrations with different weights on the peg and different bow velocities, verifying in general the conclusions already cited.

[17a] Here followed the illustration.

to and following the change of bow there was a considerable fall in the intensity as recorded by the oscillograph. I can obviate this by disciplining myself to maintaining a constant velocity right up to the point of change and then starting off immediately with the full proper velocity after the change of bow.

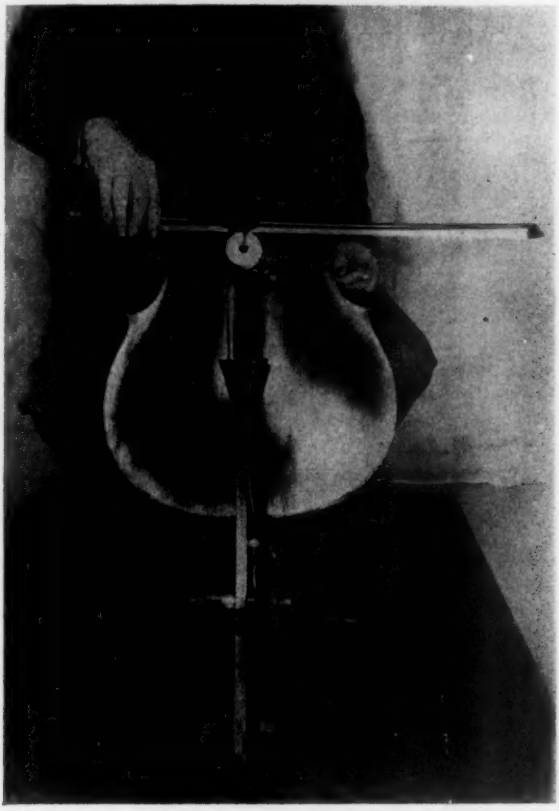

FIG. 9. A wooden bar can rotate about a horizontal axis near the floor. Its top end carries a pulley so placed that it can roll on the top of the bow as the latter is held horizontally and drawn throughout its length. Near the pulley is a peg carrying weights to determine the force with which the bow presses upon the string. The motor for drawing the bow is not shown.

I will now play a section of notes in which you will observe certain false accents. Such false accents are perhaps not always discernible to the ear of the player: but many more things are discernible to the listener than to the player.[18]

A Matter Pertaining to the Fingers of the Right Hand. There are

[18] The opening section of the Schumann A Minor Concerto was played with and without false accents of the kind cited.

Bad.
Second phalange of first finger is too horizontal. Incidentally, general wrist form bad.

Good.
Second phalange of first finger approximately in plane containing strings. It is as a matter of fact a little on the vertical side to provide for the force parallel to the string and necessary to prevent the bow from sliding down.

Bad.
First phalange of first finger approximately vertical.

FIG. 10.

several ways of holding the bow. In practically all cases, however, one of the phalanges of the finger constitutes the element by which the first finger communicates force to the stick of the bow. Since the force should be communicated in a direction perpendicular to the string which is being played, it is manifest that the first finger, and so the other fingers, should be parallel to the string, or at least they should lie in a plane passing through the string. Any other condition results in one where the first finger contributes force in a direction other than one perpendicular to its own length, and such a condition is bad because it invokes bunching of the flesh on the finger as a necessary complement of the means for producing the force, or it invites a condition in which the force is applied ultimately to the string in a direction not perpendicular thereto, and this invites, in turn, sliding of the bow towards or away from the bridge. The statement just made is made regardless of the general contour of the wrist and arm. There may be comments to be made on this matter also, but they are affairs of the professional artist and are hardly appropriate to the present lecture.

Figure 10 illustrates three ways of holding the bow, one conforming to the principle above cited, and the other two departing from it in opposite senses.

CERTAIN PSYCHOLOGICAL MATTERS IN RELATION TO MUSCULAR ACTIVITY IN THE PLAYING OF STRINGED INSTRUMENTS.

It may seem out of the field of the man of science to talk about technical matters in the playing of instruments, particularly to an audience composed in part of some who have devoted their lives to these matters. However, there are one or two things born of a kind of way in which a physicist thinks which may be of interest even to the professional musician. If any agree with what I say, I shall be delighted. If any disagree, but are interested, I am content. If all disagree and are uninterested, I apologize.

I suppose that most people would agree that such muscular activities as are concerned in the playing of a violin or a 'cello are controlled more by the nervous reflexes than by the muscles themselves, and that the muscles will respond more readily to a well-formulated pattern of intent controlled by the mind than they will to an automatic memory of procedures developed by untold hours of practice with the mind more or less on vacation. For my purpose, I shall take as an illustration the playing of what is called a staccato on a stringed instrument. For those who are not musicians, I will simply say that a staccato is a detached series of notes played in a single bow, up-bow or down-bow. It represents a piece of technique which presents a formidable obstacle to many players.[19] Some have a natural staccato. It was more or less born

[19] At this point the "staccato" was illustrated on the 'cello, by up-bow and down-bow scales.

with them at the time they took up the instrument and the best thing is to leave them alone. Some never have any staccato and they are apt to maintain that unless the staccato is born with the player, he will never acquire it, and that if he does acquire it, it will be of little use anyhow. It is a characteristic of mankind to decry the usefulness of that which he does not possess. Then there is a very large group who suggest that you should go on practicing this, that, or the other for a long time, and if the Good Lord looks with favor upon you, someday you will wake up and find that you have a staccato.

Now having been born one of the poor wretches who had no staccato, I listened to all sorts of advice and tried all sorts of dodges. The more you would analyse the thing in the normal way, the worse it would get. I would say: "I must put my hand thus and I must move it thus, and then I must follow it up with the arm, and then I must readjust the fingers"; and having gotten all this down, one would have something which he could write very nicely on paper but which failed when he came to try it. Or he might get to a place where sometimes the staccato would come off and sometimes it wouldn't. He would get so that it would behave more or less nicely when he was playing at home, on certain occasions when he happened to feel particularly well, but every impedimentum in the form of an audience would cause things to blow up, and nothing sounds more lamentable than a blown-up staccato.

It is for the foregoing reasons that I sought some simple principle of adjustment of one's psychology in terms of which the staccato would become a relatively simple operation rather than a sequence of complicated muscular adjustments performed between each note played, and I shall describe the principles which I have found more or less successful, not so much for their musical value, as for giving an illustration of one case, at any rate, where a proper psychological adjustment can serve a useful end.

If you ask what physics has to do with what I am talking about, I will say, much, I expect, to your surprise, that the foundation of this principle of playing staccato is to be found in the theory of relativity. Now, do not be alarmed, I do not intend to fill the blackboard with higher mathematics, and tell you that that is the way to play staccato. I am concerned rather with the psychological adjustment which the true relativitist favors in relation to things which happen in a moving system. Parenthetically, I remark that if for the moment I become a pure physicist, I have to say that I am not very much in sympathy with some of the matters in question in relation to the theory of relativity itself. However, for my immediate purpose they are of value and so, stifling for the moment my critical conscience as a pure physicist, I will swallow them lock, stock and barrel, and talk as though I believed them. I believe, in fact, that they are probably more true for the musical case in

which I intend to apply them than they are, from the most sophisticated standpoint at any rate, in pure physics.

Now you know that the theory of relativity maintains that if you perform an experiment upon a moving platform such as a ship, you will get the same results as if you performed it on a fixed platform, so long as, in the former case, your experiment is concerned only with apparatus on the ship and does not make use of observations on the external surroundings. Of course, the theory is usually talked of in relation to bodies such as astronomical bodies, which are moving with very great velocity in space, and the upshot of everything is to the effect that if you are on such a body there is absolutely no experiment which you can do which can tell you whether you have or have not an absolute velocity in space. By looking at your surroundings on the outside, you can conclude that you are moving *relatively* to other things, but you cannot say whether it is the other things which are moving and that you are fixed or whether it is you who are moving and the other things are fixed. The theory of relativity, in fact, favors the abolition of the idea of absolute motion. If you are on one body, it is appropriate for you to forget all about any uniform rectilinear motion which a mid-Victorian appraisal of the situation might suggest that you have and speak only of things in relation to your own frame of reference, just as you would have spoken in the mid-Victorian sense if you had believed yourself to be at rest. It is not consistent with the spirit of relativity that you should believe that you are really in motion and ought to obtain special results in your experiments as a result of that motion, were it not that these effects are compensated by some other action resulting from the motion. You are invited simply to forget all about the motion and not complicate your brain by extraneous things. An out and out relativist would say that it is wrong to think about anything other than what you experience in your own frame of reference. You see, in a sense the principle of relativity is a process of getting oneself into a state of mind—a frame of reference—in which things are simple rather than complicated.

And now let me apply the spirit of this thought to the bow, and to the 'cello. First, however, let me make an intermediate introduction. Suppose I swing this pointer back and forth.[20] That, to me, is an easy thing to think of, I have no difficulty in the matter, and the shadow of the rubber ball moves back and forth on the screen. If as I do this, I walk along, you observe that in each swing the ball does not come back as far as it goes forward because I am walking forward. If I walk fast enough, the ball will not come back at all. It will appear to you to go

[20] The text at this point will be understood from the following additional remarks. The speaker held a long pointer vertically. On the end of the pointer was a large rubber ball. The optical lantern cast a shadow of the pointer and ball upon the screen. When the speaker swung his arm back and forth parallel to the screen, the shadow of the ball moved back and forth. When he walked parallel to the screen, a combination of the two motions was realized.

forward, then slow up, stop, then go forward again, slow up, stop, and so on. In other words, you observe the rather complicated motion of a staccato, a starting and stopping of this shadow, as part of the continuous motion of my body in one direction, supplemented by a simple back and forth motion of the hand. Now, I do not have to think of all this, I just swing my hand back and forth in the simple motion. If I did try to think of the thing from your standpoint, and say: "My hand is now going forward, now it is getting slower, now it is stopping, now it is going forward again," I should have such a complicated set of adjustments in my muscles that I might have to practice ten years before I could do at all that which I am now doing so easily. I should be as badly off as the poor centipede, who, in trying to give an answer as to the order in which he moved his hundred legs, got into such confusion that he fell into the ditch and died.

Now let me turn to the 'cello. It is an easy matter for me to perform this motion [21] in any part of the bow. If now I transfer my mind, as it were, to my arm and move the arm gradually forward,[22] I have the impression of producing a very simple motion on a moving platform, that platform being my arm, but to you who sit still, my hand is really making a motion consisting of a number of startings and stoppings, if the velocity is great enough. If the velocity is only small, then, of course, I shall hear the back and forth motion on each oscillation of the hand, but as I increase the velocity of my arm, I eventually reach a point where the hand never goes back as viewed from your standpoint, and the sound produced suddenly breaks into the staccato.[23]

The main thing which I wish to emphasize in the psychology of this matter is the necessity of thinking of the simple back and forth motion only, and then superposing upon that simple back and forth motion the simple steady motion of the arm.

I may say that once the staccato has been acquired by the process I have indicated and the muscular reflexes have become acquainted with it, it is no longer necessary to meditate continually upon the two processes to which I have referred. At this stage, in fact, the staccato has become a natural one, and so strong is the control which the psychology of the situation makes upon the muscular movement, that it carries over into other types of staccato, such as flying staccato, in which the bow proceeds by a jumping action along the string and which,

[21] The motion illustrated was a simple back and forth motion of the wrist with the arm fixed, so that the average position of the bow remained fixed.

[22] Here the arm was allowed to move forward uniformly, while the back and forth motion cited in footnote [21] was maintained.

[23] This was illustrated by moving the arm slowly forward while maintaining the back and forth motion cited in footnote 21. Each back and forth motion gave rise to a separate sound. Then as the velocity of the arm was gradually increased, the sounds suddenly halved in number per second, and assumed the character of the staccato.

nevertheless, in its best form, is not limited to an undefined speed of playing, but can be accommodated in such fashion as to produce a definite number of notes in an assigned interval.[24]

The Vibrato. I should like to make a few remarks concerning what dynamics has to say in relation to the performance of a vibrato, which is an oscillation of pitch produced by a motion of the bone of the finger tip back and forth parallel to, or slightly inclined to, the string within the limits permitted by the flesh of the finger tip, which is in more or less firm contact with the string. There are other kinds of vibrato, for example, a rolling kind, in which the hand performs a kind of rotary motion on the finger tip as a pivot. I shall concern myself with the first form of vibrato to which I have referred. The motion of the finger tip is, of course, associated with, and indeed promoted by, the motion of the remainder of the hand or of the hand and arm, these motions being produced by the muscular action of the appropriate muscles concerned. We have, in fact, a fairly big mass, the hand or arm, which is pulled back and forth in a plane under conditions in which the motion of the finger tip is limited by the contact with the string.[25] The motion of the more massive portions can be greater than that of the finger tip on account of a certain elasticity, or rather give, in the fingers themselves.

Now when a mass is set in oscillatory motion by a force originating, for example, in the muscle in the present case, it accommodates its motion in such a manner that its velocity is a maximum when the force applied is a minimum, and the velocity is zero when the force is a maximum.[26] Now, two things may happen to disturb the poor 'cellist. If he is not a physicist, he is apt to be puzzled by feeling that his muscles exert least effort at the instant when the arm velocity is greatest. He may feel momentarily that this should be somehow or other rectified. Such a thought will disturb the timing of the muscular activity, and the regularity of motion of the mass will consequently be disturbed, producing a kind of physiological quarrel between the mass and the muscles, in which the former demands to know what it is that the latter really intends to do. However, there is a much more dangerous source of catastrophe. In what I have said so far, I have implied that the mass concerned is the forearm with the hand attached to it in such a manner as to form a rigid entity. Now if, during the vibrato, the rigidity of connection between the hand and arm should become momentarily

[24] At this point, flying staccato and controlled staccato with bouncing bow were illustrated on the 'cello.

[25] The muscles pulling the forearm are attached thereto, and to the bone of the upper arm slightly above the elbow.

[26] This was illustrated by a weight hanging on a spring which was pulled up and down. The force at each instant was then determined by the extension of the spring. For convenience, the weight was suspended in oil to slow up the motion. While this complicates the experiment in principle, the complication was irrelevant for the purpose of illustration in hand.

destroyed, we should be dealing with two masses instead of one, the forearm controlled by its muscles and the hand more or less independently controlled. There would be two different kinds of vibrations fighting for the privilege of controlling what happens, and the result of such a condition is an evolution of formless contortions in which all sense of regularity is lost.[27]

Thus, I would say that the lesson which dynamics teaches in relation to the vibrato is that if one wishes to have a vibrato which is determined by the hand and forearm moving as one piece, then he must see to it that they continue to move as one piece and that the rigidity of connection between hand and arm is not destroyed. On the other hand, if by any chance he should intend to produce a vibrato in which the hand alone is the moving entity concerned, then he must see that he does not allow his muscles to become inveigled into operating the forearm as a kind of competitor, for directly there is a competition for independent action between these two, catastrophe is invited.

There is a kind of vibrato, characteristic of beginners, in which all the muscular system of hand and arm becomes stiffened up, probably as a kind of defense against allowing the hand and arm to become disunited muscularly, but in which the muscles become stiffened to produce a kind of very rapid trembling motion which may well be characterized as the vibrato for paralytics. About this vibrato I have nothing to say other than that it should be avoided.

(*To be continued.*)

[27] This was illustrated by repeating the experiment cited in footnote 26, but with an additional spring and weight attached to the bottom of the first weight in order to enhance the complexity of the motion.

THE AUTONOMOUS FIELD.

BY

GUSTAF STRÖMBERG

Mount Wilson Observatory, Carnegie Institution of Washington, Pasadena, California.

Of late our ideas about force fields have undergone a great change. The first important change was introduced by Einstein and was an essential part in the general theory of relativity. Even more radical changes in our ideas were found necessary as the result of the development of modern wave mechanics. Studies of the epistemological foundations of force fields in general have also thrown much light on the subject. One result of these studies has been that many scientists have become reconciled to the idea that fields may be nothing more than sets of generalized rules expressing space and time relations in mathematical or geometrical language. Although the generalization of the field concept has made it rather abstract, it is, nevertheless, admirably suited to explain, or rather to picture, a host of phenomena which were completely intractable on the basis of the older field concept. These new ideas have made it necessary to revise our classical concepts about the causal relationship between matter and its associated field. This revision, which entails a complete reversal of old ideas, has made it possible to apply the field concept to a number of biological phenomena, in particular to the progressive organization during embryonic development. With every new application, the field concept has grown in importance, it has become an *autonomous regulator of events*, and as a consequence we now begin to see not only the world of matter but also the world of life in a new perspective.

For a long time most people thought that the earth "pulled down" all objects near its surface, and that electrically charged or magnetic bodies "compelled" other bodies in their neighborhood to move in certain ways. Tracing the effects to the elementary particles of matter, scientists pictured them as attracting or repelling one another and in general to be the cause of accelerated motions. Great difficulties were encountered when attempts were made to explain the high concentration of electric charge on exceedingly small particles and to understand the behavior of these particles at very small distances. Faraday had called attention to the fact that the field itself had specific properties, and he was the originator of *field physics*, in contradistinction to *particle physics*. The relativity theory describes a field physics in which the field is *guiding* the path of light beams and bodies in space and time according to certain rules. An observable "event" occurs when two

such paths ("world lines") cross. A field is hence observed as a "pattern of events," and from such patterns the properties of guiding fields are derived. These properties can be described as due to "field potentials," which vary continuously from one point in space-time to another. As in classical mechanics a characteristic property of the field is the potential energy, which is regarded as a continuous function of the coordinates.

Both in the classical field theory and in the theory of relativity we must regard the particles as the cause of the field, provided we define a cause as an event that regularly precedes another in a physically well defined manner. The particles are the "cores" of the field, and any motion of these cores, or any change in them, is *later* manifested in the field properties. If the cores are neutralized, as, for instance, by the junction of positive and negative charges, the external field is progressively dissipated as radiation. The cores can hence be regarded as the "sources" of the field, and a field without sources of some sort is regarded as unthinkable.

In the further development of the general theory of relativity the distinction between particles and their fields has tended to disappear. The possibility of a fusion of fields and particles is well described by Einstein and Infeld [1], who have given an excellent description of the development of the field concept. In such a fusion, however, the field tends to become the sole governing agency, and then we find it difficult to understand how the particles have come into existence and why their high energy concentration does not cause them to explode on the slightest provocation. The authors write (pp. 257–258): "We cannot build physics on the basis of the matter concept alone. . . . Could we not reject the concept of matter and build a pure field physics? . . . There would be no place, in our new physics, for both field and matter, field being the only reality. . . . Our ultimate problem would be to modify our field laws in such a way that they would not break down for regions in which the energy is enormously concentrated. . . . The decision, as to whether it is possible to carry it out, belongs to the future." In the following we shall see how, in general terms, at least, such a program can be carried out; what is the origin of the particles and what role they play; and what are the general consequences of such a theory.

Our knowledge of matter and fields is mostly based on observations of large-scale phenomena, and our theories have been developed primarily to represent such phenomena. For instance, the idea of identifiable bodies moving in continuous paths independent of any observation is inherent in the classical theory. When attempts were made to apply this idea to processes involving individual particles and transfers of

[1] "The Evolution of Physics," Simon and Schuster, 1938.

small amounts of energy, it was found that this simple picture of motion was not applicable. It appeared that both light-beams and moving electrons had a *particle aspect* and a *wave aspect*, the former associated with the mass and the energy involved, the latter describing the place and time of their appearance as well as certain directional and vibrational properties. The only satisfactory theory for the elementary processes was based on the ideas characteristic for the quantum theory, that energy always appears as indivisible units depending only upon an associated frequency, and that the place and time of its appearance can only be predicted with a certain degree of probability. Large-scale phenomena must therefore be assumed to be governed by statistical laws, but since in general we are dealing with a large number of particles, these laws are, for most practical purposes, rigorous.

Although the dual aspect of matter and of radiation is now familiar to all physicists, it has not been properly emphasized that this dualism cannot be reconciled either with our classical ideas about particles as the sources of force fields, or with the idea of fields as guiding particles in continuous motion, whatever be the sources of the field, or with that of a simple fusion of particles and fields, as mentioned above. For instance, the diffraction of an electron beam reflected by a metallic crystal shows that the "thing" reflected does not consist of indivisible particles and does not itself carry any energy as such. Instead, it must be regarded as a field having a *coherent structure* in space and time defining the *probability* that energy in observable form will appear at a given place and at a given time.[2] The diffraction can only be explained by assuming that the interfering parts of the wave system contain time elements originating in the *same* emission process, since these are of very short duration and cannot have mutually dependent phase relations. It is evidently incorrect to say that electrons are emitted as such, since they do not travel as identifiable objects through space at definite speeds. It should be noted that the whole physical space, in the usual method of representation, is needed to represent the probability structure of each of them, an idea familiar from other problems dealing with systems containing many electrons.

The probability pattern or field element associated with an individual emission process has a structure the meshes of which, as in the case of electro-magnetic radiation, have a size or wave length depending upon its velocity relative to the observer who measures the properties of the field. The field of an electron that appears to be almost stationary relative to the observer possibly has no observable meshes, only a structure of spherical or, for a spinning electron, axial symmetry. This electrostatic field should not be regarded as being caused by a "little

[2] A detailed description of this and other types of coherence is given in my article, "Coherence in the Physical World," *Philosophy of Science*, **9**, 323, October, 1942.

particle" at its center, imparting peculiar properties to the surrounding space by some mysterious mechanism nobody has ever been able to explain or understand. We may say that the field element defines, for different regions, the chance of observing an "electron in action." Since a stationary field has only one point where the probability is a maximum, we may picture the associated electron as *emerging*, if it emerges at all, close to but not necessarily at the geometrical center of the field. The probability structure associated with an individual "electron emission" forms an *integral, coherent field unit* in which the sum total of probabilities is unity. In the present field theory the indivisibility of particles is regarded as a necessary consequence of the existence of integral field units defining units of probability structures.

A satisfactory feature of the present theory is that electrons which do nothing else than travel from one place to another in our mental pictures are eliminated as unobservables. Carrying this thought further we arrive at the conclusion that continuous motion is a mental abstraction that has no exact counterpart in the realm of physical measurements. For individual particles, motions are derived from *events* separated by *finite* space and time intervals. (This applies also to electron tracks in a Wilson cloud chamber, as shown in the last mentioned article.[2]) Applied to composite bodies, motion has statistical significance only. The product of the wave length of a field unit and its associated momentum, and also of the observed energy amount and the inverse value of the frequency, which itself can be regarded as a measure of duration of the emergence, all measured by a *particular* observer using "tools" (atoms, light) built of similar field units, equals the quantum of action. Its finite size puts an upper limit to the accuracy of his measurements of position and velocity of the particle. This is the essence of the Principle of Uncertainty. The present theory gives quantitatively the same result, but it also tells us that the concept of motion should not be applied to the particles themselves, which are regarded as ephemeral, energy-carrying indicators of the field structure and not as identifiable bodies in continuous motion.

The same idea can be applied to ordinary radiation and gives a good understanding of the wave nature and particle nature of light. Also in this case the thing emitted consists of a system of integral, coherent structures, which can be described as wave motions. Each of these field units determines the probability that, at a given place and at a given time, a definite amount of energy and linear and angular momentum—properties which, if highly localized, we habitually regard as characteristic of a particle, in this case called a *photon*—will be absorbed. If no absorption occurs, the coherent structure associated with an individual emission process expands in an *ever widening surface* with a certain thickness defined by the length of the wave train. This is strikingly shown when we observe the interference of two beams

of light from a distant star. The two interfering beams, although separated in space by many feet and travelling over enormous distances, contain time elements having their origin in the *same atom* and in the *same emission process*. This can be immediately deduced from the facts that the two beams have fixed phase relations and that the individual emission processes are of very short duration.[3] The probability structure due to such a process instantaneously disappears as a whole, whatever its size may be, when the full energy it controls is absorbed by an atom. If we regard such structures as nothing else than abstract mathematical probabilities, there is nothing strange in this, and a mathematical physicist may be completely satisfied with such a picture. But probability is a highly complex mathematical construct, and as such cannot travel from one place to another. It is natural to regard a probability as being "carried" or "caused" by an underlying, active entity of which we have only an incomplete knowledge. The instantaneous disappearance of an enormously extended probability structure suggests strongly that our ordinary idea about separation and motion is highly artificial and that a more "natural" representation should be adopted. Such a representation is offered by the space-time manifold of the relativity theory. Expressed in space-time intervals the separation between a radiating atom and the field unit it emits remains zero, and the field unit does not expand at all. Since the interferometer is used to measure the position of something (loci of maximum probability, usually called wave crests) travelling with the speed of light, the length in space-time of the interferometer base is also zero.

The coherent field unit emitted in a radiation process can be regarded as a representative part of the electronic envelope of the emitting atom. Instead of saying that a photon has been emitted by one atom and hits another, we might say that an integral portion of the field structure of the emitting atom is temporarily "in contact" with an atomic field, and an incorporation of the whole field unit with its coherent wave system can take place. This occurs especially when the two atoms have similar structures, so that a field unit from one atom fits into the structure of the other. Such a phenomenon we call absorption of radiant energy. It might be pointed out that the reason we think of radiation as travelling through space at a definite speed is associated with an intrinsic characteristic of the human consciousness, namely of "experiencing the world" as time sections and not in its true aspect as a space-time manifold. If we think in terms of the latter the difficulty of understanding action at a distance can be eliminated. When we see

[3] Cf. "Coherence in the Physical World," *loc. cit.* Dr. F. Zwicky has called my attention to the fact that in induced radiation several atoms can emit radiation with fixed phase relations. Since the primary impulse comes from a single atom, the origin of the coherence can even in this case be traced to a single emission process, the other atoms acting, so far as phase relations are concerned, as reflecting mirrors.

a distant star, integral portions of its atoms of long ago are in contact with nerve ends in the retina of our eyes, and through nerve fibers with optic ganglia and with our brain, and a contact is established with our perceiving mind.

It is clear from what we have just said that in the case of those fields, which we ordinarily describe as electron beams and radiation, it would be incorrect to say that the electrons or the photons are the "cause" of the fields or that they are "sources" defining the structure of the fields. This follows directly from the facts that the idea of moving particles leads to logical contradictions and that *a field determines the location of particles* and not vice versa. In the first decade after Bohr's theory of atomic structure was enunciated we used to think of electrons moving about the atomic nuclei in discrete orbits. It was later found that this simple model must be abandoned and should be replaced by another describing an *energy pattern* with sharply defined energy levels, and that the pattern itself could be described mathematically as a vibrating system. It cannot be too strongly emphasized that this vibrating system does not consist of moving particles or of a vibrating body, but is simply a convenient mathematical representation of a probability structure of the same type as that needed to characterize electron beams. The logical impossibility of regarding an electron beam as caused by moving particles applies equally well to the energy pattern around an atomic nucleus. Particles are descriptive terms applicable to observable *events* in space-time. These events are in general highly localized in space as well as in time and involve definite amounts of mass and energy, and it is for these reasons that in many cases we can describe them as ephemeral particles.

We can apply the same reasoning to atomic nuclei. The heavier nuclei are identifiable bodies, which in a certain sense can be "labeled." That an atomic nucleus can be regarded as a probability pattern or energy pattern, similar to the structures describing the electronic envelopes surrounding them, is clearly shown in the case of radioactive elements. We are therefore justified in stating that the properties of atomic nuclei are not due to any elementary particles residing or moving inside the nuclei, but to an inherent energy pattern the structure of which can to a certain extent be deduced from a study of certain events observable as a momentary emergence of definite amounts of energy and momentum. (In this case the idea of *emergence* of definite energy amounts, determined by the structure of a probability pattern, appears perfectly natural. The present theory implies a more general application of this idea.)

It is often stated that the probability patterns or wave systems described represent our knowledge of past events and can be used to predict future events with certain degrees of probability. The knowledge acquired gradually loses its value for prediction as time goes on,

suddenly to become exact knowledge (that is, as exact as nature will permit) when a new observation is made. According to this theory the coherent wave systems are creations of our mind and have no more objective reality than a "wave of optimism spreading over the country." The wave systems of which we have spoken have definite space and time properties, however, which give them practically the same degree of truth value as that of most other statements concerning the physical world. It is easily seen that they are of exactly the same nature as the radio waves that often precede the wave of optimism. Radioactive phenomena show clearly that our picture of the structure of an atomic nucleus does not depend upon our knowledge or ignorance of previous emission processes or upon the accuracy with which such processes have been measured. Although the structure of atoms is a creation of the human mind, it is not an arbitrary creation. It has a truth value of a special type, and the truth it represents is, as always, incomplete. It represents a partial aspect of nature, mirrored in a mind of a particular type that is trained to think along certain lines. The full truth describing what goes on in the atomic nuclei, as in other physical processes, is not revealed by any observations of the type used in our laboratories and probably does not belong to the world of space and time with which physics is alone concerned. But more about this later.

Summarizing, we may say that force fields should not be regarded as being caused by any particles, but are *autonomous* entities built of *indivisible, coherent units* defining probabilities in space and time. The particles are nature's own evanescent *indicators* of the fine structure of preexisting fields and are associated with *emergence* of definite amounts of energy into the physical world of space and time. Atoms and matter in general are not aggregates of particles, but can best be described as energy patterns defining the amount, the vector properties, and the probable place of the emergent energy at a given time. Potential energy and probability of emergence of energy are almost synonymous concepts. (The problem of the origin of the energy-carrying indicators and of the field structures will be discussed later.)

A simple illustration may help us to get a clearer picture of the new ideas. We are used to think of an electric current in a wire as being due to electrons moving predominantly in one direction, the flow being the result of forces due to a potential difference. The characteristics of the field are assumed, at least to some extent, to be due to the inherent properties of the moving charges, which are considered the actual cause of the field. In the autonomous-field theory there are no moving electrons, and there are no visible sources of the field, visibility being regarded as a consequence of the properties of certain fields. The strength of an electric current is determined by the number of integral field units passing a cross section of the wire per unit of time. As in

atoms, a potential difference is a measure of the number of the energy levels separating two energy states. Each field unit has its own extension of the same type as that we usually associate with a moving electron. These field elements carry no energy, only probabilities. To change the probability structure into observable properties, mass, in the form of "squirts" of energy of fixed amounts, must enter and annul the structure. Only then and thus can be demonstrated the existence of what we call an electric current and its field. (As in the case of light, we might say that the emergence of action entails the disappearance of the corresponding "action cells." This correspondence between energy and probability structure secures the conservation of energy, not for each instant, but during finite time intervals.)

It is easily seen that, in spite of their unfamiliarity, these ideas give in many ways a more satisfactory picture of an electric current than that involving moving particles and the fields directly attributed to them. Impedencies have the dimension of angular momenta. Resistance is measured by the number of action units irreversibly transferred to the atoms of the resistor. Inductance has the dimension of a moment of inertia and acts like a flywheel spinning around the conductor carrying the current. A capacity is represented by the number of field units compressed in the condenser plates and the dielectric by unit voltage. The idea of integral field units determining the emergence of definite units of energy, as well as the particle aspect and wave aspect of matter and electricity, are all explicitly expressed in the picture. The last two appear, not as usual as alternative aspects, never to be applied in conjunction, but as concomitants with a definite causal relationship. Since an electric current is here pictured as a coherent probability pattern, it follows that a more accurate form of Ohm's law should contain definite phase relations. We can therefore expect interference phenomena when an electric current is divided and enters at several points on the surface of a perfectly uniform conductor, e.g., a metallic crystal at very low temperature.

The theory of the autonomy or primacy of all force fields is of fundamental importance in physics. Its most immediate importance, however, lies in the fact that it can be directly applied to certain fundamental problems in biology. A new factor is then introduced into this science, and many hitherto completely inexplicable phenomena appear in a new light. Even the age-old problem of the relationship between mind and matter is no longer insoluble and will ultimately take its appropriate place in a revitalized science.

If we ask what it is that determines the structure of an organism, the answer usually given is that in the germ cells of the animals and plants are certain chemical substances of a highly complex type, which determine the future development. Some of these substances (nucleo-

proteins) can be traced to the nuclei of the germ cells, and specifically to the chromosomes that undoubtedly carry many of the hereditary characteristics of the organism in a latent form. Some of these substances occur as indivisible units called genes, which are situated in the chromosomes and reproduced in cell division when the chromosomes are split and re-formed. The direct connection between the morphogenetic factors in the cells and the physical development of the organism is still unknown. Transplantation experiments made during the early gastrula stage show that organizing factors can be transferred from one part of an embryo to another. Of special interest in our study is the fact that certain organic tissues, even when killed, can induce, or rather initiate, cell differentiation. In short, it is stated that the origin of form and function in organisms can be traced to chemical substances, some of which are of a relatively simple type and can be regarded as releasing dormant qualities in the cells of which living matter is built. Since in all atomic and molecular structures distance effects are supposed to be due to electric fields, the conclusion commonly drawn is that organization in the living world is due to such fields, and, what is more important, that the properties of the fields are ultimately determined by the configuration and motion of electrically charged particles arranged in stable structures.

As long as electric fields were regarded as the only effective cause of motion (gravitational fields being too weak to be of importance) and as long as electric fields were supposed to be "anchored" in particles, this explanation was the only one that could be reconciled with our classical field concepts. It was soon realized, however, that ordinary physical fields were completely inadequate to explain the observed facts. A more general field concept was therefore introduced by several prominent biologists to explain organization. H. Spemann, A. Gurwitsch, P. Weis, De Beer, and Hurley [4] have all envisaged organizing fields in the developing embryos, although in most cases the ideas are descriptive rather than explanatory. Gurwitsch [5] is most explicit. He states that "The place of the embryonal formative process is a field (in the usage of the physicists) the boundaries of which, in general, do not coincide with those of the embryo but surpass them. Embryogenesis, in other words, comes to pass inside of the field. . . . What is given to us as a living system would consist of the visible embryo (or egg, respectively) and a field. . . . The question is how the field itself evolves during the development of the embryo."

Several years earlier Hans Driesch in his Gifford Lectures of 1907 and 1908 had developed similar ideas. In describing the fields he used

[4] A summary of these field concepts is found in Spemann's excellent book: "Embryonic Development and Induction," Chapter XV, Yale Univ. Press, 1938.

[5] "Ueber den Begriff des embryonalen Feldes," *Archiv für Entwickelungs Mechanik*, 51, 392, 414, 1922.

the Aristotelian term "entelechy" which he supposed to have non-physical as well as physical properties. Driesch's theory was unacceptable to biologists, partly because it was "tainted" with vitalism and partly because everybody was convinced that all fields were completely determined by atomic configurations.

The idea of an organizing field independent of atomic structures and extending beyond the boundaries of an embryo has recently been given strong support from the work of the Section of Neuro-Anatomy of the Yale Medical School. Burr and Northrop [6] proposed in 1935 the "electro-dynamic theory of life." This theory induced Burr, Lane and Nims to construct the "vacuum-tube microvoltmeter," [7] an instrument particularly suited for the study of the fine structure of the electric fields around animals and plants. By this method "it has been possible to explore the electrical properties of a wide variety of living forms undisturbed by changes in resistance in the organism and without disturbing in any significant way the inherent electrical properties of the thing measured." Many important facts have been established by this group of workers. It is sufficient here to state that the electric gradients are not chaotic, but exist in a well-defined pattern characteristic of the species to which the animal belongs. In some cases it has been definitely shown that the pattern extends beyond the organism. At death the field disappears. Burr [8] writes that "it is hard to escape the conclusion that the electrical pattern is primary and in some measure at least determines the morphological pattern."

The fields studied are called *electric* because they can be detected by electrical measuring instruments and apparently can influence the motion of electrically charged molecules (ions). But these fields obviously must be of a type quite different from those associated with a combination of atoms of the chemical elements and called chemical substances. It is inconceivable that such substances, however complex they may be, can be responsible for the organization in living matter. A characteristic feature of living organisms is their stability of structural details, and, in animals in particular, the stability of the pattern of organization of parts with definite functions. This stability is directly shown by the unchanging forms in a long series of generations. Fluid chemical substances of the type with which we are dealing (e.g. proteins) are in general highly unstable. In the theory of the autonomous field, *stability is a characteristic of a field structure* and cannot be attributed to any particular configuration of particles. For instance, the fields which define the properties of atomic nuclei usually have a

[6] *Quart. Rev. Biol.*, **10**, No. 3, 322–333, Sept. 1935.

[7] A summary of the work up to 1939 is given in *Proc. Nat. Acad. Sci.*, **25**, No. 6, 284–288, 1939.

[8] "Field Properties of the Developing Frog's Egg," *Proc. Nat. Acad. Sci.*, **27**, No. 6, 280, 1941.

very high stability, formerly described as an immutability of chemical elements. We know now, from a study of radioactive phenomena and of artificial transmutation of chemical elements, that atomic nuclei with different degrees of instability exist. Similarly, the fields associated with organization have different degrees of stability, a fact manifested by the existence of mutations and the emergence of modified living forms. When it is considered that certain parts of living systems, at least in man and in the higher animals, must be regarded as carriers of or at least associated with a consciousness, as is the cortex of our brain, the idea of chemical substances being responsible for organization and function appears absurd to many people. Such substances furnish the necessary energy, however, and often exert a trigger effect.

For convenience I have used the term "living fields" as a more general term than "organizing fields" and "embryonic fields."[9] A simple case of an integral living field is offered by the transformation of a blastula into a gastrula. It is known that in this case the cells passively follow a field of force extending over the whole embryo, and that, when a part of the gastrula is transplanted into new surroundings, it may for a time continue to change its shape in the way it started to do. The latter phenomenon shows that living fields can be "cut" without immediately losing their inherent properties. The problem of the reproduction of living fields by splitting has been discussed elsewhere.[10]

When we say that force fields are autonomous we mean that they are not caused by particles, like electrons and protons. It is obviously difficult to picture a field which can not be attributed to something else. We naturally look for a cause of the field; something beyond the observable effects. This "something" must be of a very profound nature, since it must in some way or other be associated with the ultimate origin of energy, matter, life, and mind. It is customary to use the term "sources" as denoting the origin of force fields, and I have used the term "living sources" to designate the sources of the living fields. Such sources of infinitesimal size exist in the germ cells, and, when the sources expand, each of them becomes an integral living field with a definite and effective structure. In my earlier publications I regarded the particles themselves as the sources of the atomic and the molecular fields. This is only partly correct, since the particles are momentary sources of energy, but they are not the cause of the field structures. Although the heavier particles of matter define the center of a field with high accuracy, the wave nature of these particles shows that they define, not the exact geometrical centers of fields, but regions with statistical

[9] A detailed description of the properties of such fields is given in my book, "The Soul of the Universe," David McKay Company, Philadelphia, 1940. This book also describes in simple terms many of the ideas here mentioned and the scientific facts which support the same.

[10] "The Physical and the Non-Physical World and their Intermediate Elements," *Scientific Monthly*, **54**, 71, January, 1942.

properties. In searching for the cause of force fields we realize immediately that an important element is missing in the theory.

The missing element is furnished by the introduction of the non-physical world. In the last mentioned article [10] I have given the reasons for the assumption of the existence of such a world. Our distinction between a physical and a non-physical world can be traced to the fact that we observe nature by different sense organs, the physical world being based on our organs of vision and of touch. The non-physical world is everywhere contiguous with the physical world of space and time, a notion familiar from modern geometry of many dimensions. But the new "dimension" cannot have space properties, that is, it cannot be measured as intervals and structures, although incipient spatio-temporal, that is, physico-chemical, properties associated with vital functions can be determined at the points where it comes into contact with or emerges into the physical world of space and time. These contact points I have called *intermediate elements*, and they are the connecting links between the physical and the non-physical world as pictured in our type of consciousness. It is through these contact points that *energy*, *organization*, and *mind* temporarily enter into the world of space and time. In space-time they appear as physical fields built of elementary field units and observable as atoms and matter, plants and animals, nerve cells and integrated nerve systems with brains, respectively. In the autonomous-field theory the corpuscular nature of matter, the interference of light, the diffraction of particles, and the existence of structural elements and of an integrated organization in living organisms, are all regarded as evidence of different kinds of indivisible field units. The astonishingly small size of the intrinsically enormously complicated organizing factors in the germ cells is a natural consequence of the fact that they emerge from a world to which our ordinary space conceptions are not applicable.

In his book "The New Background of Science" (Macmillan, 1933), Sir James Jeans has expressed a very similar idea. He writes on page 259: "We are thus led conjecturally to think of space and time as a sort of outer surface of nature, like the surface of a deep flowing stream. The events which affect our senses are like ripples on the surface of this stream, but their origins—the material objects—throw roots deep down into the stream." [11]

By a study of different types of living fields we can obtain some idea of the characteristics of the non-physical world. In the paper [10] mentioned I have studied the living fields responsible for the structure of

[11] W. F. G. Swann, in his book "The Architecture of the Universe" (MacMillan, 1934), describes on page 415 how two worlds could be connected by linkages of certain types. These linkages correspond to my "intermediate elements." In several of his publications, Dr. Swann has expressed ideas about the nature of particles which are somewhat similar to those expressed in this paper.

the optic ganglia in the retina, a study which gives us a clue to the origin of our sensation of colors. We shall here study another type of living fields that gives us information about another aspect of the non-physical world. The shape and structure of muscles from the standpoint of the present theory must be regarded as due to living fields. A muscle is a fluid structure, and it has no great rigidity when dead. Its changes in shape and in rigidity, both important characteristics of an autonomous field, are often associated with nerve impulses. The coherent and rhythmic contraction of the heart muscle, for instance, indicates the existence of a living field of unitary character. The pulse is determined by certain nodal nerve cells connected to the brain by the vagus nerve. The pulsation can be stimulated by certain chemicals like adrenaline. Excised portions of heart tissue can be kept alive, and the individual cells will pulsate under certain conditions. The nerve connections in the heart are necessary for a coherent pulsation of the heart as a whole and should therefore be regarded as a visual manifestation of the links in a more general field than those exhibited by the individual cells. Whatever conception we have of how the heart has been developed in the animal world, there can be no doubt that, at present, it performs a very essential function in certain animals, and that its motions and structure, linked as it is with the arteries, veins, and capillaries, are admirably adapted to perform this particular function.* We may well say that the heart exhibits a purposeful action although we are not consciously aware of its purpose, which we have known for only a relatively short time. This observation indicates that one of the characteristics of the non-physical world is *purposeful activity*.

Another indication of the same thing is offered by those muscles the activities of which are associated with a *conscious will*. In a consistent materialistic school of thought there can be no place for a will, as an independent and effective agency. Adherents to this school therefore claim that will is an epiphenomenon that accompanies certain muscular activities, but not others. This, however, is contrary to our experience, since we are well aware of a will, or at least a desire to do something, without the will being accompanied by any appreciable muscular action. In other words, the will does not seem to accompany muscular activities, and it certainly does not follow them. Instead, all our experience tells us that it *precedes* them, and it can therefore well be regarded as the *cause* of action. According to the autonomous-field theory, the living field in our nervous system is modified and directed by something in our own consciousness which we call will, a process which does not by itself introduce any added energy into the muscle. Since will does not have any space properties, it belongs not to the physical but to the non-physical world, although it is in intimate association with the field structure. Again we find that in the non-physical world there is a *purpose*, which we perceive as will when it originates or appears in our

consciousness. Whether or not the will is free to act is a question that cannot be answered, because we cannot trace mental activities to their ultimate origin. The only thing we can say about its origin is that it cannot be located in material particles, although these provide the energy necessary for its physical manifestations.

These ideas are in complete harmony with those expressed recently by R. S. Lillie. He regards living systems as "psychophysical," that is, they are assumed to have both physical and psychological characteristics. I quote from one of his articles [12] entitled "Living Systems and Non-Living Systems": "In the adult animal (e.g. a man) the functioning of the whole organism as a biologically well adapted unity is demonstrably dependent in large part on psychological factors. It is true that these have their physiological correlates. Nevertheless, such general facts remind us, whenever we are inclined to forget, of the special character of the living organism as not merely a physical but a psychophysical system; and they show the scientific insufficiency of a purely physical conception of the living organism." If we accept the idea that *all* organisms have psychological as well as physical properties, will may play a much more important part in the development of the living world than we have thought.[13] Although this will only reaches our consciousness in special cases when the proper nerve connections are present, we can still regard it as an active force, provided we accept the theory of the autonomy of force fields. But since we do not know to what degree, if any, our will is "free," we cannot answer the question whether the will, whose effects we perceive, is that of the individual or of a more general "cosmic will." Personally I believe it is a combination of both.

In conclusion we can say, that the theory of the autonomy of force fields leads to the result that such fields can be analyzed into integral, coherent units, and that this theory then makes it possible to understand many phenomena which the classical field theory is unable to explain. Principal among these phenomena are the wave nature and particle nature of matter and radiation and the small-scale and the large-scale organization in the living world. The theory has been shown to be a natural consequence of modern wave mechanics. It is also in harmony with recent discoveries in biology and gives us a new concept of life. It is directly opposed to the mechanical concepts which have until recently characterized natural science. It points directly to a world intimately connected with our own consciousness.

[12] *Philosophy of Science*, 9, 319–320, 1942. See also his latest article "Vital Organization and the Psychic Factor," *Philosophy of Science*, 11, 161, 1944. In these and previous articles Professor Lillie has emphatically expressed the opinion that the organization in the living world cannot be reconciled with ordinary physical laws. The autonomous-field theory developed from an attempt to explain the organization in the living world and at the same time to understand the origin of consciousness.

[13] Cf. "The Soul of the Universe," p. 221, 222.

MAXIMUM EMULSION SPEED IN RELATION TO THE DEVELOPING AGENT.

BY

T. H. JAMES.

Communication No. 985 from the Kodak Research Laboratories.

The photographic latent image can be evaluated at present only in terms of its ability to induce development. The quantity which is measured directly (density or silver) is dependent upon the development process. However, the densities obtained at "maximum development" may vary with the developer used, resulting in variations of the photographic characteristics such as gamma and emulsion speed. Thus, Nietz [1] found an eighteenfold variation between emulsion speeds obtained with the best and the poorest developing agents used under the particular conditions of his experiments. Shiberstoff and Bukin [2] obtained a much smaller variation for the same agents. The following comparison of the two sets of relative speeds shows a lack of agreement both in magnitude and order:

	Nietz	Shiberstoff and Bukin
Methyl *p*-Amino-*o*-Cresol	100.0	—
Pyrogallol	76.0	30.1
Hydroquinone	46.0	25.1
Eikonogen (1-amino-2-hydroxynaphthalene-6-sulfonic acid)	18.0	31.5
Elon (methyl-*p*-aminophenol sulfate)	10.0	22.7
p-Aminophenol	7.6	17.6
Adurol (chlorhydroquinone)	6.3	29.7
Catechol	5.5	26.8

Evidently, the variation is not uniquely dependent upon the developing agent, and the comparative values suggest that Nietz was using some of the agents under conditions far removed from the optimum.

In the present paper, a comparison is made of emulsion sensitivity and gamma values obtained with various developing agents used under certain simplifying conditions. Development was carried approximately to completion, and the nature of the solutions was such as to confine development largely to the surface latent image. The development times usually ranged from 1 to 2 hours. Comparative values were also obtained for development by two conventional high-speed elon-hydroquinone developers. Two typical motion-picture positive

[1] A. H. Nietz, "The Theory of Development," Monograph No. 2 on the Theory of Photography, Eastman Kodak Company, Rochester, N. Y., 1922.

[2] V. I. Shiberstoff and Yu. I. Bukin, *Kino-Photo-Ind.*, 1932, p. 101.

emulsions were used, since in a preliminary investigation of this type it appeared desirable to use relatively simple emulsions of low inherent fogging tendencies. Exposures were made on the Eastman IIb sensitometer. Log E for the 11th step was 0.55. The exposure changed by a factor of $\sqrt{2}$ for each step, and the 21st step corresponded to the lowest exposure used.

The developing agents were used in caustic solution (with the exception of the potassium ferro-oxalate developer) and in the absence of sulfite. The use of a high pH value (12.7) permitted comparisons under nearly equivalent conditions for all agents employed. Most of these agents are ionized in alkaline solution, and the ions constitute the active developing agents. The concentration of ions at any given pH depends upon the ionization constants for the particular agents, and there is considerable variation among the constants. In some cases, the pertinent constants are known only to a very rough approximation, and accurate calculations of ion concentrations at moderate pH values cannot be made. At pH = 12.7, however, all the agents are either completely ionized or largely in the ionized form. In every case, more than half of the added developing agent is in the form of ions at pH = 12.7.

Omission of the sulfite, which is usually present in conventional developers, introduces a second simplification. The primary function of sulfite in the developer is to prevent the formation of stain and to retard the oxidation of the developing agent by air. However, sulfite also exerts a solvent action upon the silver bromide, and this can introduce complications. One result of primary importance to the present investigation is that sulfite promotes fog. In general, this action is greater the longer the time of development and the lower the bromide content of the developer, but it varies with the developing agent. A comparison of the developing properties of hydroquinone and elon in the presence of considerable sulfite places the hydroquinone at a distinct disadvantage, because the relative effect of sulfite upon fog formation is greater with hydroquinone. A considerable increase in emulsion speed (and often in gamma) can be obtained by omitting the sulfite from a hydroquinone developer. This is illustrated in Fig. 1, where the basic composition of the developer was as follows: hydroquinone, 8.8 gm. per liter; sodium carbonate, 48 gm. per liter; potassium bromide, 0.40 gm. per liter. This is a representative formula for a conventional developer, except for the absence of sulfite. Figure 1 gives a comparison of results obtained with this developer, and with one of like composition to which 48 gm. per liter of sulfite had been added.

The absence of sulfite necessitates working under the exclusion of oxygen. All development operations were carried out under an at-

mosphere of nitrogen, using a procedure previously described.[3] In some of the developers used a definite stain image was formed, which complicated the measurement of density. In most cases, however, the stain image was found to be nearly proportional to the total density. Correction was made by bleaching out the silver and reading the density of the stain strip superimposed on a sensitometer strip which had been developed to approximately equal silver density in a potassium ferro-oxalate solution. This reading, after the original density of the strip,

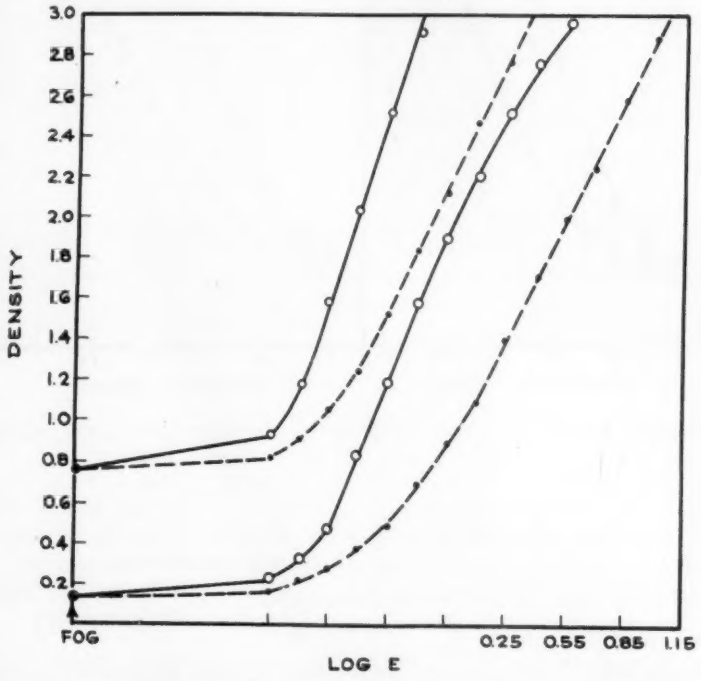

FIG. 1. Effect of Sulfite on Development by Hydroquinone: —·—·— = "Normal" Sulfite; -o-o-o- = No Sulfite. The two sets of curves correspond to two sets of development times.

developed in ferro-oxalate developer, had been subtracted, gave the "stain density." Such a correction is only approximate, but is sufficiently accurate when the stain correction is small. Usually, the stain was small, although it amounted to about 14 per cent. of the total density for the exceptional case of catechol.

Speed was determined in terms of the inertia point. The speed values obtained agree quite closely with minimum useful gradient speeds for the particular emulsions used (with a short toe, and long straight-line portion, cf. Fig. 2). Since the stain correction for the various exposure steps was approximately a constant fraction of the

[3] T. H. James, *J. Phys. Chem.*, **43**, 701 (1939).

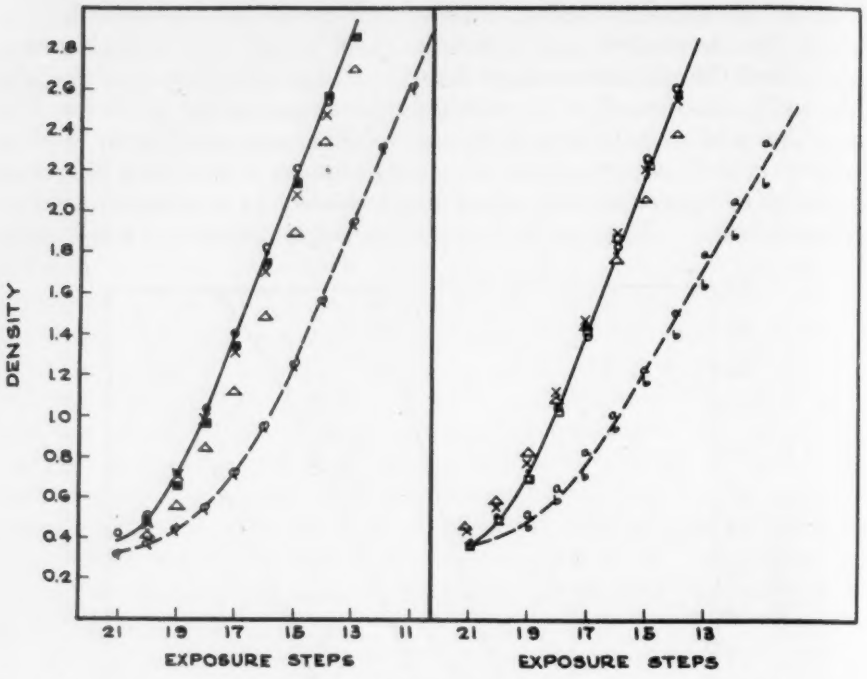

FIG. 2 (a) and (b)—Characteristic Curves Obtained by Development to a Fog Density of 0.30:

(a) —·—·—	D-19	(b) —·—·—	D-19
o o o	$Fe(C_2O_4)_2$	o o o	Elon
x x x	Elon monosulfonic acid	x x x	Diaminodurene
φ φ φ	Hydroxyphenylglycine	△ △ △	Catechol
■ ■ ■	E−H	■ ■ ■	*p*-Amino-*o*-cresol
△ △ △	Hydroquinone	a a a	*p*-Aminophenylglycine
		b b b	Hydroxylamine

total measured density, stain corrections made little difference in the measured speed values. The corrections are of importance, however, in determining the gamma values.

CRITERION FOR MAXIMUM DEVELOPMENT.

Ideally, a developer should reduce only those grains which contain one or more latent-image nuclei. It should leave unaffected all grains which receive no exposure. Such a developer does not actually exist. The available developers merely reduce the "sufficiently exposed" grains at a substantially greater rate than the unexposed or insufficiently exposed ones (apart from those grains which are spontaneously developable as emulsion fog). The ultimate experimental differentiation is determined by the point at which the rate of fog formation equals that of image development.

The efficiency of development at any particular stage is determined

by the ratio of the rate of development to the rate of fog formation.[4] If *Di* is the image density and *Df* the fog density, the respective rates can be measured conveniently as $Ri = dDi/dt$ and $Rf = dDf/dt$. The efficiency of development then can be expressed as Ri/Rf. For all practical developers, this ratio must be high in the early stages of development and in the normal exposure region. The value decreases with decreasing exposure, in the low-exposure region, and becomes substantially unity at some sufficiently low exposure. The ratio for any specified exposure in the normal useful range also decreases as development proceeds. When the ratio drops to unity, useful development is at an end for that particular exposure. If development is prolonged, the ratio eventually drops below unity with the result that the distinction between the exposed and unexposed areas *decreases*. These changes are illustrated in Table I.

TABLE I.

Determination of "Maximum Development."
Diaminodurene Caustic Developer.

Exposure Step	*Ri/Rf* Values at Various Fog Densities					
	Fog	0.06	0.13	0.43	0.71	1.20
21		1.67	1.42	—	1.00	0.97
19		3.0	2.17	1.13	1.05	.97
17		6.0	3.75	1.73	0.95	.91
15		13.0	5.0	2.10	.89	.75

The point of maximum development gives the maximum distinction which can be obtained experimentally between the exposed and unexposed regions. For this reason, it is significant in latent-image studies. For practical photographic work, however, it is generally desirable to stop development well below the maximum, since the high fog attending maximum development is usually objectionable.

EXPERIMENTAL RESULTS.

A series of preliminary measurements showed that nearly optimum development could be obtained with most of the agents tested when they were used in a solution of the following composition: developing agent, $0.005M$; excess potassium hydroxide, $0.04M$; potassium bromide $0.00667M$. (This solution was decidedly unfavorable for certain other agents, such as pyrogallol and amidol, which fogged badly in it.) Somewhat higher emulsion speed was obtained with hydroquinone when used in the carbonate formula given previously, but this was an exception, and other agents tested in the same formula, particularly elon, glycine, and catechol, gave somewhat poorer results. The formula was

[4] C. E. K. Mees and C. W. Piper, *Phot. J.*, **51**, 226 (1911).

not particularly sensitive to bromide concentration, which usually could be varied severalfold without affecting the maximum emulsion speed. Development was carried out at 20° C.

Table II gives data for a comparison of emulsion speeds when development was carried to the maximum point. Data for two standard elon-hydroquinone developers are included: the Kodak D-19, and the "E–H." The latter had the composition: elon, 3.0 gm.; hydroquinone, 15.0 gm.; sodium sulfite, 45 gm.; sodium carbonate, 56 gm.; potassium bromide, 2.0 gm.; and total volume, 1 liter. The ferro-oxalate developer contained only 0.002*M* potassium bromide.

TABLE II.

Comparison of Developing Agents at Maximum Development.

Agent	Relative Speed	Agent	Relative Speed
Hydroquinone (carbonate)	100.0	*p*-Amino-*o*-cresol	100.0
Catechol	97.8	D-19	93.3
Ferro-oxalate	95.5	Hydroquinone (caustic)	89.9
Elon	100.0	*p*-Aminophenylglycine	72.5
Elon monosulfonate	95.5	Hydroxylamine	72.5
Diaminodurene	95.5	*p*-Hydroxyphenylglycine	69.3
E–H	95.5	Ascorbic Acid	64.6

With the exception of the last four agents listed, the data show only a small variation in speed. It is even doubtful if the variation listed for the first eight is genuine, since the accuracy with which the speed values were determined did not exceed about 5 per cent.

A more complete comparison was made when development was carried to a fixed fog density of 0.3 in every case. (The fog density at complete development usually lay in the region of 0.7 to 1.2.) The results are quite similar to those for complete development. Numerical

TABLE III.

Development Carried to Fog Density of 0.3.

Agent	Relative Speed	Gamma	*W*	Per cent. Stain Correction
Diaminodurene	100.0	2.55	255	0
Hydroquinone (carb.)	100.0	2.60	260	4
p-Amino-*o*-cresol	97.7	2.65	259	4
Catechol	97.7	2.30	234	14
p-Aminophenol	95.5	—	—	—
Ferro-oxalate	95.5	2.56	244	0
Elon	93.3	2.70	252	4
D-19	93.3	2.56	239	0
Elon monosulfonic acid	91.2	2.54	232	Trace
Hydroquinone (caustic)	83.2	2.64	220	7
Hydroxylamine	77.6	1.68	131	0
p-Aminophenylglycine	74.1	1.81	134	2
p-Hydroxyphenylglycine	64.6	2.42	156	3
Ascorbic acid	40.8	2.58	105	0

results are given in Table III. The values for W are obtained by multiplying speed by gamma, and correspond to Shiberstoff's "degree of perfection." Figure 2 gives a plot of the characteristic curves of the various developers. Some comparisons were also made with a second, higher-gamma motion-picture positive emulsion. This material showed even less dependence of speed upon the developing agent, the relative speeds varying from 100 for diaminodurene to 91.2 for caustic hydroquinone (the last four agents listed in Table III were not used with this emulsion).

The variation in speed and gamma for the first eight developers listed in Table III is quite small, even though the variation in chemical nature is marked. The fact that elon and hydroquinone yield nearly the same values is of special interest in view of the rather common belief that hydroquinone is inherently a "weak" developer. The gamma value for catechol apparently is abnormally low owing to the action of

TABLE IV.

Relation of D/Ag for Fog and for Image Development.

Agent	Fog Density	D/Ag	Ratio: $\frac{D/Ag \text{ (fog)}}{D/Ag \text{ (image } D = 2.5)}$
Hydroquinone	0.41	77.9	1.04
Elon	.33	87.6	1.11
Catechol	.31	81.8	1.14
Ferro-oxalate	.25	71.0	1.03
Diaminodurene	.30	80.2	1.03
E–H	.31	76.1	1.08
	.62	77.0	1.10
Ascorbic acid	.81	76.1	1.01
	2.01	80.7	1.06

the oxidation product which destroys the developability of certain grains in the higher exposure steps. The oxidation product produced during the development of some of the grains may simply have destroyed small development centers of neighboring grains which had not yet started or had just started to develop, thus preventing subsequent development of those grains. A destructive action of this type was observed previously in some experiments with hydroquinone at low pH values (8.0–8.9), where quinone was responsible for the effect.[5] A similar action in the present case is indicated by the fact that addition of sulfite to the catechol developer produced a substantial increase in gamma for equivalent development, providing development was not carried to the point of serious fog.

The preceding comparisons were all based upon density measurements. In order to determine whether or not the quantitative results held good for developed silver as well as for density, sample silver

[5] T. H. James, *J. Phys. Chem.*, **44**, 42 (1940).

determinations were made by potentiometric titration with iodide. The values of D/Ag were then determined, using densities corrected for stain. No attempt was made to distinguish fog density from image density, but D/Ag values for fog silver were determined for the various developers.

Some results are shown graphically in Fig. 3, where the ratio D/Ag is plotted against density. The silver is expressed in milligrams per 100 sq. cm. of film. All data apply to development carried to, roughly, a fog density of 0.3. It is seen that the various developers yield similar curves, the slopes being generally the same, but the curves for the

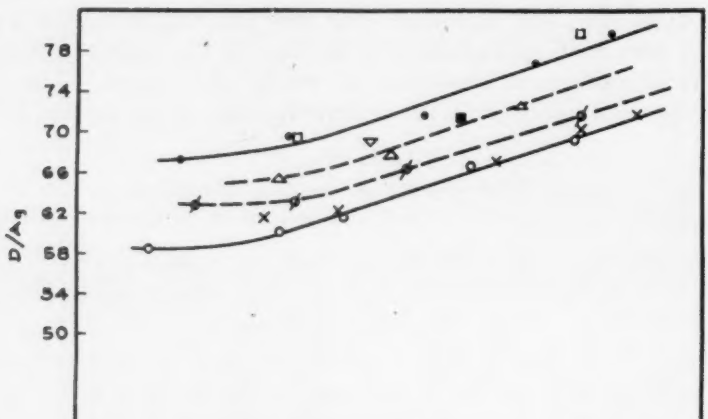

FIG. 3—Relation of Silver to Density:

o o o	$Fe(C_2O_4)_2$
x x x	E–H
—·—·—	Diaminodurene
△ △ △	Hydroquinone
ϕ ϕ ϕ	Catechol
□ □ □	Elon
▽ ▽ ▽	Glycine
■ ■ ■	Elon monosulfonic acid

various developers being displaced somewhat along the D/Ag axis. Silver determinations were likewise made on fog strips. Table IV gives the results in terms of the D/Ag values for specified fog densities, and also in terms of the ratio of fog D/Ag to image D/Ag, where the latter applies to a density of 2.50. The ratio of fog to image D/Ag changes by only a few per cent. among the various developers. Thus, little change would be introduced into the comparative data of Table III and Fig. 2 if silver values were substituted for density measurements.

The fact that several of the developing agents used under the present

simplified conditions yield emulsion speed and gamma values which are equal to or slightly greater than the values obtained with two high-speed developers of normal composition is significant. The simple developers should be primarily surface developers, and have little action on the internal latent image. Sulfite, the primary solvent, is absent. Agents such as hydroquinone, catechol, and ferro-oxalate have no solvent action on the silver bromide. Moreover, the amount of potassium bromide used in excess reduces the solubility of silver bromide in water to nearly the minimum. This is shown by the following data, which were obtained at 25° C.

Silver bromide is soluble in pure water to the extent of 0.156 mg. per liter.[6] Additions of small amounts of bromide decrease the solubility, until a minimum of 0.008–0.009 mg. per liter is attained in the region of bromide concentration of 0.0002–0.001 M. Beyond the minimum, the solubility increases. At 0.0015 M Br^-, it is 0.010; at 0.0067 M, it is 0.022. Thus, the optimum development in the caustic solution was obtained at a bromide concentration corresponding to somewhat less than three times the minimum solubility of silver bromide, but to only one-seventh the solubility in pure water. A variation in solubility of 4.5-fold, produced by changing the bromide concentration from 0.002 M to 0.020 M, had very little effect upon the speed and gamma values obtained, however, which indicates further that the solubility factor is of little importance under the present conditions.

The close correspondence between the results obtained with the surface developers and with the normal elon-hydroquinone-sulfite combination suggests that the internal latent image either is not effective for the latter, or that surface image is always found on grains containing internal image [7] *in the particular emulsion used*. It is noteworthy that addition of hypo and other solvents which should uncover internal latent image [8] did not increase the emulsion speed obtained with the E–H developer. In fact, the additions often produced a decrease in speed and a marked change in the ratio D/Ag.

The close correlation between gamma and speed values for a number of developing agents of widely different chemical constitution indicates a sharply defined minimum exposure required for developability. *The critical size and configuration of the latent-image center which will just promote development of a given grain must be substantially the same for each agent.* On the other hand, development by hydroxylamine and *p*-aminophenylglycine apparently is not promoted by such centers. A heavier exposure is required before the grain becomes developable by

[6] A. J. Moyse, A. Ballard and S. E. Sheppard, Private communication.

[7] G. Kornfeld, *J. Opt. Soc. Am.*, **31**, 598 (1941).

[8] Cf. A. Kempf, *Zeits. wiss. Phot.*, **36**, 235 (1937); W. F. Berg, A. Marriage and G. W. W. Stevens, *J. Opt. Soc. Am.*, **31**, 385 (1941).

these agents. This may also be true for ascorbic acid and *p*-hydroxyphenylglycine, although the evidence is less clear-cut here. The fact that the speed, relative to the standard, is increasing for each agent right up to maximum development indicates that the growth of fog is complicating the situation.

The speed and gamma values obtained during the earlier stages of development may vary considerably according to the nature of the developing agent, and the disparity is often great. Thus, hydroquinone may give less than one-tenth the speed of diaminodurene or elon when development is carried to equal (low) gamma values. The early stages of development are much more sensitive to kinetic factors which affect the rate of initiation of development in the individual grains, and which may vary with exposure to a far greater extent for one developer than for another. The cause of this variation will be discussed in a subsequent paper.

SUMMARY.

Maximum emulsion speed and gamma values were obtained with a series of developing agents used under optimum conditions and in the absence of sodium sulfite. Seven of the eleven agents tested gave substantially the same values. The critical size and configuration of the latent-image center which will just promote development of a given grain must be substantially the same for each of these agents. The other four agents gave definitely lower speed values. No agent or combination of agents has been found which gives higher speed values. The conventional elon-hydroquinone developers which contain sulfite give only equal or somewhat lower values.

NOTES FROM THE NATIONAL BUREAU OF STANDARDS.*

NEW RUBBER LABORATORY IN THE AMAZON VALLEY.

Norman Bekkedahl of the Bureau's rubber section has just returned to Washington after spending two years in Brazil. In 1942 at the request of the Brazilian Government the Bureau loaned the services of Dr. Bekkedahl to direct the establishment of a modern rubber laboratory at the Instituto Agronomico do Norte located in Belem in the State of Para (*Technical News Bulletin* Nos. 308, 316 and 321). He reports that the laboratory is now completely equipped and in operation, while its staff continues to be built up. Dr. Bekkedahl was assisted in this work by Fredrick L. Downs, one-time member of the Bureau staff and later an employee of the American Steel and Wire Company of Worcester, Mass., which granted him a leave of absence.

The Instituto Agronomico do Norte is one of several research institutes supported by the Brazilian Government and is somewhat similar to the Regional Research Laboratories of the United States Department of Agriculture. Dr. Felisberto Cardoso de Camargo, the director of the Instituto, has concentrated the research program largely on rubber, since it is the most important export of the Amazon region. The location of the Instituto at Belem is particularly fortunate, since all the rubber from the Amazon valley passes through this port at the mouth of the river. Whatever the relative positions of natural and synthetic rubber in the post-war years, the Amazon Valley must be considered as one of the more important potential sources of natural rubber.

The fact that Belem is a seaport and an important stop on South American and trans-Atlantic airlines, also has made it possible for the Instituto to operate a so-called "factory" located in the jungle about 5 miles from the Instituto. Here about 100 liters of rubber latex are collected each day from wild trees and processed into raw rubber sheets.

A wide variety of types of trees, other than the common rubber tree (Hevea Brasiliensis), are under investigation. Studies of different methods of coagulation can be made under various typically primitive conditions. The rubber which is not used in experiments is sold through normal commercial channels.

Plans are being made for continued cooperation between the new laboratory and the National Bureau of Standards and other organizations in the United States for the development of improved methods of testing and grading natural rubber, and for fundamental research on rubber of different origin and treatment.

* Communicated by the Director.

ANALYSIS OF BUTADIENE AND STYRENE BY MASS SPECTROMETER.

The Consolidated Engineering Corporation's mass spectrometer, recently installed at the Bureau, is being employed in analyses of liquid and gaseous materials for making synthetic rubber. This is an elaborate and highly specialized instrument that makes possible a quick, complete, and accurate analysis of samples of butadiene, styrene, and other monomers submitted by plant operators through the Rubber Reserve Company. Since many of the constituents are closely-related hydrocarbons, the analyses by conventional methods would be impossible, or at least would involve considerable research on the method to be applied to each constituent. Studies are likewise being made of the purity of butadiene and styrene produced in different plants and by different methods. Other projects deal with the composition of recycled butadiene and styrene, which are recovered after polymerization for use in a later run. Such studies furnish information regarding the amount and kind of purification necessary for these materials. Analyses of vent gases have led to the conclusion that considerable amounts of valuable materials were being allowed to escape and steps have been taken to reduce or eliminate the waste.

PREPARING REFRACTORY OXIDES, SILICATES, AND CERAMIC MATERIALS FOR ANALYSIS.

As an outgrowth of work in preparing refractory platinum-metal alloys for analysis by treatment with acid mixtures at high temperatures, it has been found that numerous other substances which have caused trouble for the analyst will respond to a similar method of treatment. Aluminum oxide, beryllium oxide, tin oxide, cerium oxide, and thorium oxide, all of which are very resistant to the usual methods of chemical treatment if they have been ignited at high temperatures, dissolve rather readily in hydrochloric acid at temperatures ranging from 200° to 300° C. The treatment is applied in sealed glass tubes enclosed in steel pressure vessels charged with compressed carbon dioxide in sufficient amount to keep the glass tubes from bursting under the high pressures developed by the acid (TNB331, November 1944).

Most aluminum-bearing minerals, as well as a variety of other minerals and ceramic materials, can be decomposed, preparatory to analysis, by this treatment. The work is reported by Edward Wichers, William G. Schlecht and Charles L. Gordon in the December number of the Journal of Research (RP 1621).

GAGE FOR MEASURING VAPOR PRESSURES OF CORROSIVE LIQUIDS.

The experimental work reported in TNB331 (November 1944) on the use of hydrochloric acid and other acid mixtures at temperatures

up to 300° C. to prepare for analysis refractory platinum-metal alloys and numerous refractory oxides, ceramic materials, and minerals, has necessarily involved the development of suitable laboratory technics for conducting the new method of treatment. It was also necessary to measure the high pressures developed by the acid mixtures under the conditions of operation. In RP 1622 in the December number of the *Journal of Research*, Charles L. Gordon, William G. Schlecht, and Edward Wichers describe an all-glass gage, with auxiliary equipment, suitable for the approximate measurement of the vapor-pressures of corrosive liquids. They report the pressures developed by various strengths of hydrochloric acid and its mixtures with nitric acid or perchloric acid at temperatures up to 300° C. The observed pressures ranged up to 4000 lb./in.^{2}.

The paper describes how to construct, fill, and seal the glass tubes used as reaction vessels, and how to construct and use a simple steel pressure vessel to prevent the glass tubes from bursting. Carbon dioxide, added in solid form (dry ice) furnishes the compensating pressure. Information is also given on the proper amounts of carbon dioxide to compensate for the pressures developed within the sealed tubes by the various acid mixtures that are used.

HEAT OF FORMATION OF CARBON DIOXIDE.

In the *Journal of Research* for December (RP 1620), Edward J. Prosen, Ralph S. Jessup, and Frederick D. Rossini present a recalculation of data previously reported on the heats of combustion of graphite and diamond (TNB258, October 1938), together with some new data on graphite. As a result of this recent work, certain "best" values have been selected as representing the heats of combustion of graphite and diamond and the transition of graphite into diamond:

$$C(c, \text{graphite}) + O_2\,(\text{gas}) = CO_2\,(\text{gas});\quad \Delta H^\circ_{298.16} = -393{,}447 \pm 45 \text{ int. j/mole} = 94{,}051.8 \pm 10.8 \text{ cal/mole.}$$

$$C(c, \text{diamond}) + O_2\,(\text{gas}) = CO_2\,(\text{gas});\quad \Delta H^\circ_{298.16} = -395{,}343 \pm 96 \text{ int. j/mole} = 94{,}505.1 \pm 22.9 \text{ cal/mole.}$$

$$C(c, \text{graphite}) = C(c, \text{diamond});\quad \Delta H^\circ_{298.16} = 1896 \pm 85 \text{ int. j/mole} = 453.2 \pm 20.3 \text{ cal/mole.}$$

HEATS OF COMBUSTION OF BENZOIC ACID AND GRAPHITE.

Experimental data on the heats of combustion of benzoic acid and carbon in the form of graphite are presented by Edward J. Prosen and Frederick D. Rossini in the December *Journal of Research* (RP 1619). The data on benzoic acid support the change in the value of its heat of combustion reported by Jessup two years ago (TNB306, October 1942).

The data on graphite yield a slightly higher value for the heat of formation of carbon dioxide than that previously reported.

COMMERCIAL STANDARD FOR SOLID-FUEL-BURNING FORCED-AIR FURNACES.

Commercial Standard CS109-44, now available in printed form is a recorded voluntary standard of the trade for solid-fuel-burning forced-air furnaces. It was developed by the industry in cooperation with the Government, on request of the Federal Housing Administration. The standard sets forth definitions, requirements for construction, performance under test, test code, informative labeling, and a uniform manufacturer's guarantee declaring the maximum and minimum output obtainable according to the commercial standard.

The standard will provide uniform methods of testing and rating, a nationally-recognized basis for guaranteeing performance and output, and will constitute a basis for fair competition. It became effective for new production on March 10, 1944.

Copies of CS109-44 are obtainable from the Superintendent of Documents, Government Printing Office, Washington 25, D. C., at 10 cents each.

COLORBLINDNESS TESTS.

Through the committees of the Inter-Society Color Council, the Bureau has cooperated with the Army Air Forces, the U. S. Submarine Base at New London, and the Bureau of Medicine and Surgery, in trials and revisions of established tests of color vision and in the design of new tests. The purpose of this work is to assist the Navy and War Departments in their efforts to make sure that men with defective color vision are not placed where this defect would be a handicap.

About 2 per cent. of otherwise normal males lack the ability to distinguish red-green differences, and to about half of this 2 per cent., red signals not only fail to appear red but also have less than one-fifth normal brightness. Such observers are known as protanopes to distinguish them from other red-green-blind observers, to whom red signals are normally bright, known as deuteranopes. It has been found possible to express the quantitative data on the color confusions of protanopic and deuteranopic observers, obtained by Pitt a few years ago in Great Britain, in terms of the ICI standard observer for colorimetry.

The derivation of the functions defining the color confusions of average red-green-blind observers is given in RP 1618, Standard Response Functions for Protanopic and Deuteranopic Vision, by Deane B. Judd, in the December number of the *Journal of Research*. These functions have been useful in analyzing the performance of established

tests for color vision and in the design of new tests. This use of the functions is exemplified by the solution of three problems arising in design of colorblindness tests, and comparisons of these solutions are made with experimental results. It is pointed out that these functions accord with the form of three-components theory of color vision worked out in 1897 by Arthur König, and with the dominator-modulator form of that theory suggested in 1943 by Granit. These functions do not, however, form the basis for a complete theory of color vision such as that of G. E. Müller.

50 Years of Progress in Automatic Weaving.—C. W. BENDIGO. (*Textile World*, Vol. 94, No. 8.) Fifty years ago there was delivered to Queen City Mfg. Co., in Burlington, Vt., the first part of an order of 792 Northrop automatic-changing looms. This was the first commercial installation of automatic looms in the world. Automatic weaving was the product of American inventive genius protected and encouraged by our patent system. Since then, weaving mechanism has been far from static.

Automatic weaving, a term which connotes automatic replenishment of the filling supply, was not an idea that was thought out suddenly and developed at once. No basic patent was ever granted covering it. There is considerable controversy as to when the first experimental automatic loom was built, but certainly it was many years before the Queen City installation. It was probably of the shuttle-changing type, which was not made practical until after the bobbin-changing principle had been accepted.

The first bobbin-changing loom was called the Northrop loom, after James H. Northrop, who first conceived the principle. Other men, however, were involved in its development.

Previously, cloth quality was very poor as judged by today's standards. For example, a broken pick or two picks in one shed were expected, and it was common practice not to repair a warp thread until the loom was stopped to change the filling bobbin. Heavy marks and light marks were prevalent in all types of cloth, since cast gears were used throughout the loom.

Automatic weaving introduced improvements in many parts of the loom which were not directly related to the problem of replenishing filling. This was because the automatic principle sought to take the weaver away from the loom for long intervals, and it was important that the machine behave properly in his absence. An outstanding example of this related progress was the invention of the warp stop motion. Commercial development of the automatic loom was in true fact a revolutionary milestone in the history of weaving.

This 50th anniversary comes during a period of intense research and development in production machinery. In the earlier days, most machinery developments were made by inventor mechanics. This is quite in contrast to modern developments, which often are carried on by several inter-related staffs all working on the same problem. The early inventors had nothing to help them that compares to the stroboscope and electronic devices.

The stroboscope has radically affected the settings of looms, since it has shown that settings based on a manual turning over of the mechanisms are inaccurate. The kinetic forces are important, and they can be studied by means of stroboscopic flashes.

R. H. O.

THE FRANKLIN INSTITUTE.

STATED MONTHLY MEETING, DECEMBER 20, 1944.

The stated monthly meeting of The Franklin Institute was held at 8:15 on Wednesday, December 20, 1944, in the hall of the Institute. The President, Mr. Charles S. Redding, presided.

The National Anthem was played, after which the meeting was called to order by the Chairman.

Upon motion, the minutes of the October meeting were approved as printed in the November JOURNAL.

The Secretary, Dr. Henry Butler Allen, then made the following report:

Increases in membership as of November 30, 1944:

Associate Life	2
Active	56
Associate	48
Student	28
Total	134
Total Membership	5522

Dr. Allen then spoke of the annual Christmas Week Lectures for Young People on the James Mapes Dodge Foundation and announced that the lecturer this year would be Charles H. Colvin, Aircraft Instrument Designer, Engineer, and Inventor. The lectures will be given December 27, 28, 29, and the subject, "Flying by Instrument." He stated tickets were on sale at the Institute.

In accordance with the By-Laws, Article IV, Section 4, nominations of officers and certain members of the Board of Managers should take place at this meeting.

Dr. Allen, in agreement with these regulations, then read the following names which were submitted by members of the Institute:

President
(to serve one year)
CHARLES S. REDDING

Vice-Presidents
(to serve one year)
WALTON FORSTALL
W. CHATTIN WETHERILL
SAMUEL S. FELS
RICHARD W. LLOYD
RICHARD T. NALLE

Treasurer
(to serve one year)
M. M. PRICE

Members of the Board of Managers
(to serve three years)
EDWARD G. BUDD
HIRAM S. LUKENS
ORUS J. MATTHEWS
MORTON GIBBONS-NEFF
GEORGE WHARTON PEPPER
GEORGE P. REA
JAMES S. ROGERS
CLARENCE TOLAN, JR.

(to serve two years)
RALPH KELLY

(to serve one year)
RUPEN EKSERGIAN

The Chairman then asked if there were any nominations from the floor.

No other names were presented; therefore, upon motion, duly seconded and carried, the nominations were declared closed.

The Chairman then introduced the speaker of the evening, Maurice Eisenberg, eminent 'cellist, formerly Professor of the Pablo Casals 'Cello Class, Ecole Normale de Musique, Paris; now Head of the 'Cello Department, Philadelphia Musical Academy; who spoke on "The Emotional Aspects of Musical Performance as Related to the Physical and Instrumental."

Mr. Eisenberg gave a very informative talk on the rendition of musical compositions involving the three elements—the composition itself, the instrument, and the player, and pointed out the significance of the coordination of these three elements. His talk was illustrated by motion pictures and by the playing of selections by Mr. Eisenberg on the violoncello.

The enthusiastic audience was dismissed with a rising vote of thanks to the lecturer.

HENRY BUTLER ALLEN,
Secretary.

NEW MEMBERS OF THE FRANKLIN INSTITUTE ELECTED, MAY 1, 1944, TO DECEMBER 1, 1944.

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ASSOCIATE LIFE MEMBERS.

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Mr. Truman W. Read
Mr. Merville Willis

SUSTAINING MEMBERS.

Mr. Frank Tracy Griswold

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Mr. Daniel B. Altman
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Mr. M. S. Baskin
Mr. Charles B. Beck
Michael J. Bennett, M.D.
Mr. Paul E. Blouch
Mr. Samuel D. Brown
Mr. David L. Buchanan
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The library and reading room are open on Mondays, Tuesdays, Fridays and Saturdays from nine o'clock A.M. until five o'clock P.M., Wednesdays and Thursdays from two until ten o'clock P.M.

RECENT ADDITIONS.

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NOTES FROM THE BIOCHEMICAL RESEARCH FOUNDATION.

Abstract of **The Biochemistry of Vibrio cholerae. I. Growth Methods.**—RICHARD W. LINTON AND ROBERT K. JENNINGS (*Archives of Biochemistry*, **3**: 419, 1944). A simple liquid medium capable of producing large crops of cholera vibrios has been developed. K_2HPO_4, $MgSO_4$, and $(NH_4)_2SO_4$ supply the mineral requirements, while glucose and a small amount of tryptic digest of casein supply the necessary organic material. Ammonium sulfate also aids in restraining acidification of the culture during growth. Heavy growth is obtained when aeration is carried out vigorously throughout the period of incubation. The medium may be completely removed from the completed culture by dialysis and evaporation.

Abstract of **The Biochemistry of Vibrio cholerae. II. The Influence of Environmental Factors on Growth.**—ROBERT K. JENNINGS AND RICHARD W. LINTON (*Archives of Biochemistry*, **3**: 429, 1944). The behavior of the organisms in the salts-casein-digest medium described in paper number I, in regard to pH changes and glucose utilization, was studied. Glucose was found to regulate the final crop within certain limits. Growth was enhanced in proportion to the glucose present up to 0.3 per cent. original concentration. More than this amount was not utilized, the acid forming from this much glucose abruptly terminating the growth. Aeration was found to promote heavy growth more as a result of supplying proper concentrations of dissolved gases than as a result of the stirring effect. Abnormally high CO_2 content in the aerating gases favored growth.

Abstract of **The Synthesis of the Ethyl Esters of Some 2-Oxo- and 2-Thio-1,2,3,4-tetrahydro-5-pyrimidinecarboxylic Acids.**—DONALD W. MCKINSTRY AND ELIZABETH H. READING (*Journal of the Franklin Institute*, **237**: 203, 1944). Some ethyl 2-oxo-4-R-6-methyl-1,2,3,4-tetrahydro-5-pyrimidinecarboxylates and several of the corresponding 2-thio-compounds were prepared by a modified Biginelli condensation.

Abstract of **The Study of Typhoid Antigens by Electrophoresis. I. Immunological Reactions.**—RICHARD W. LINTON, LOUIS DESPAIN SMITH, AND LAURA E. KREJCI (*Archives of Biochemistry*, **4**: 195, 1944). Heavy killed-cultures of typhoid in a dialysable aerated medium were concentrated by evaporation *in vacuo*, freed of cells by high-speed centrifugation, and dialysed free of the non-specific chemical components. Electrophoretic analysis of these preparations in the Tiselius

apparatus revealed three components present, whether the preparation was derived from H, O, or Vi-containing strains. Each of the three fractions provided some protection against infection with virulent typhoid organisms to mice, but were not in any way superior to the unfractionated concentrate. The fractions obtained from the Vi-containing strains were most potent in this respect. It was also noted that while only the gamma fraction showed serological activity as indicated by precipitin reactions in the case of the H and O strains, with the Vi strains the beta component was also serologically active. Indeed, it appeared that the Vi antigen was associated with this fraction.

Abstract of **The Biochemistry of Vibrio cholerae. III. Acid Regulation by Means of the Carbon-Dioxide-Bicarbonate Buffering System.** —ROBERT K. JENNINGS AND RICHARD W. LINTON (*Archives of Biochemistry*, **4**: 311, 1944). As an outgrowth of the studies reported in the previous paper a still more satisfactory culture method was devised. Relying on the minerals carried over in a large inoculum of salts-C-D culture for inorganic requirements the new BRF medium had only to supply glucose and a small amount of amino acids (in the form of casein-digest) as nutrient matter. Sodium bicarbonate added to this substrate permitted the establishment of a CO_2-exchange buffering system by regulating the CO_2 content of the aerating gases. This made possible the utilization of much more glucose without appreciable pH drop, and concomitant increase in the crop of vibrios. The final culture is extremely dense. It may be killed with phenylmercuric nitrate and used directly as a vaccine without further manipulation.

Abstract of **The Ribonuclease Activity of Pasteurella pestis (Plague Bacillus).**—GLADYS E. WOODWARD (*Journal of Biological Cehmistry*, **156**: 143, 1944). Analytical data, obtained from hydrochloric acid precipitation and uranium fractionation, show that yeast nucleic acid is enzymatically decomposed by living cells of *Pasteurella pestis*, cells killed by phenylmercuric nitrate and by a cell-free preparation. Only part of the nucleic acid decomposed is hydrolyzed to mononucleotides, the remainder probably existing in a depolymerized state. The decomposition is accompanied by liberation of only a trace of inorganic phosphate.

All of the enzymes of the ribonuclease system are inactivated somewhat by heat, the least inactivation of the depolymerase being produced at pH 6.5 and of the tetranucleotidase and mononucleotidase at pH 7.6.

Tables of Numbers Related to the Tangent Coefficients.—H. M. TERRILL AND ETHEL M. TERRILL. The numbers K_n may be defined by

$$K_n = 2(2^{2n} - 1)B_n, \qquad (1)$$

where B_n is the nth Bernoulli number, e.g. $B_5 = 5/66$. The numbers K_n have been attributed to Genocchi by Lucas ("Theorie des Nombres," Paris, 1891, p. 251) who gives a small table of them.

These numbers are integers and have important mathematical applications in several branches of analysis. It may be noted that $(-1)^n K_n$ is sometimes denoted by G_{2n}. An extensive theory of these numbers has been elaborated by Bell (*Transactions of the American Mathematical Society*, **28**: 129, 1926; **31**: 405, 1929). Lacunary recurrence formulas for their computation have been given by Lehmer (*Annals of Mathematics*, **36**: 637, 1935).

The numbers H_n may be defined by

$$nH_n = 2^n(2^n - 1)B_n. \tag{2}$$

An equivalent definition is

$$2^{n-1}H_n = T_n, \tag{3}$$

where T_n is the nth tangent coefficient, in the notation used by Peters ("Zehnstellige Logarithmentafel," Vol. 1, Anhang, Berlin, 1922). The H_n are known to be integers (Nörlund, *Acta Mathematica*, **43**: 121, 1922; Chowla, *Messenger of Mathematics*, **57**: 122, 1927).

Since

$$nH_n = 2^{n-1}K_n, \tag{4}$$

the numbers H_n and K_n may be readily checked against each other. Other methods of checking are also available, however.

We define the numbers $K_n{}^p$ for positive values of p and n by the recurrence formula

$$p^2K_n{}^p - (p+1)^2K_n{}^{p+1} = K_{n+1}{}^p \tag{5}$$

with the initial values $K_2{}^p = 1$, $p = 1, 2, 3$, etc. Following is a small table of $K_n{}^p$.

n \ p	1	2	3	4
2	1	1	1	1
3	−3	−5	−7	−9
4	17	43	81	131
5	−155	−557	−1367	−2729

As an example of (5), we have

$$K_5{}^2 = 4K_4{}^2 - 9K_4{}^3 = 4 \cdot 43 - 9 \cdot 81 = -557.$$

The numerical values of K_n may be checked against those of $K_n{}^1$ which occur in the first column.

In similar manner we define the numbers H_n^p by the recurrence formula

$$2p^2H_n^p - (p+1)(2p+1)H_n^{p+1} = H_{n+1}^p \qquad (6)$$

with initial values $H_2^p = 1$, $p = 1, 2, 3$, etc.

n \ p	1	2	3	4
2	1	1	1	1
3	−4	−7	−10	−13
4	34	94	184	304
5	−496	−2008	−5200	−10702

As an example of (6) we have

$$H_5^2 = 8H_4^2 - 15H_4^3 = 8 \cdot 94 - 15 \cdot 184 = -2008.$$

The numerical values of H_n may be checked against those of H_n^1.

The recurrence formula (6) is related to one given for the generalized Euler numbers by Terrill (*American Mathematical Monthly*, **44**: 526, 1937).

Values of K_n are given in Table I and those of H_n in Table II.

For the ratio of two successive values of H_n, we have

$$H_n/H_{n-1} = \tfrac{1}{2}T_n/T_{n-1}. \qquad (7)$$

Thus the ratios T_n/T_{n-1}, given by Peters (*op. cit.*), are immediately

TABLE I.

n	K_n
1	1
2	1
3	3
4	17
5	155
6	2073
7	38227
8	9 29569
9	288 20619
10	11096 52905
11	5 19432 81731
12	290 51510 42481
13	19132 96724 83963
14	14 65562 61547 68697
15	1291 88508 84480 17715
16	1 29848 16368 11073 01953
17	147 61446 73378 41640 01387
18	18845 15541 72881 86751 12649
19	26 84635 31464 16547 14826 81379

TABLE II.

n	H_n
1	1
2	1
3	4
4	34
5	496
6	11056
7	3 49504
8	148 73104
9	8197 86496
10	5 68142 28736
11	483 54473 17504
12	49581 24445 83424
13	60 28356 44995 62496
14	8575 63496 14189 40416
15	14 11083 01927 54881 49504
16	2659 30677 61890 77543 99744
17	5 69062 45479 13405 71761 70496
18	1372 26233 93637 76229 91313 96096
19	3 70400 54732 70641 75559 76856 53504

applicable to the table of H_n. Also, it can be shown that K_n/K_{n-1} equals approximately $\frac{1}{4}E_n/E_{n-1}$ where E_n are the Euler numbers and the ratios E_n/E_{n-1} are given by Peters.

Further,

$$H_n/H_{n-1} = \frac{2(n-1)}{n}K_n/K_{n-1}. \qquad (8)$$

Thus, as n becomes large, one ratio is approximately twice the other.

BOOK REVIEWS.

PRODIGAL GENIUS, The Life of Nikola Tesla. 326 pages, 15 × 22 cms. New York, Ives Washburn, Inc., 1944. Price $3.75.

This biography of Nikola Tesla was written by the Science Editor of the *New York Herald Tribune*, a friend of Tesla's for many years. Data for the work were obtained from many sources including that from Tesla himself before his death in 1943 at the age of 87 years.

Nikola Tesla was born in 1856 in the hamlet of Smiljan, in the Austro-Hungarian province of Lika, now a part of Yugoslavia. The father was pastor of the village church and the mother, although totally unable to read or write, was nevertheless an intellectually brilliant woman. The child inherited culture developed and passed on by ancestral families that had been community leaders for many generations. Early in his life he showed definite signs of ability which were further developed in his attendance at the Gymnasium, the Polytechnic Institute at Gratz, Austria, and the University of Prague. Then followed various positions in Europe, none of which offered opportunity to bring forth and make practical his idea of the generation, transmission, transformation and utilization of alternating currents. Later, in America, this opportunity came. It resulted in the sale of his many patent rights to his polyphase system to George Westinghouse. In subsequent years, his inventions and discoveries were covered by more than 100 patents.

Tesla gave many lectures before scientific and engineering societies in America and Europe. In 1893, he lectured before the Franklin Institute and in 1894 he received the Elliott Cresson Medal of the Franklin Institute for the development of alternating currents of high frequency. The author stated that Tesla in 1898 demonstrated in Madison Square Garden a boat operated by radio control, and the first practical system of wireless transmission. This system he had described fully in 1893, anticipating other inventions in the field. In 1899 he built a laboratory in Colorado Springs from which he produced the first bolts of man-made lightning, and transmitted power through the earth without wires to a distance of 26 miles. In 1892 he demonstrated the first electronic tube. He was the pioneer in developing modern phosphorescent, fluorescent, and neon tube types of electric lighting. The Tesla coil is one of his many contributions to the science of radio, and other researches laid the foundations for the modern science of electronics.

The book is written in a fascinating, narrative style. It provides easy, interesting, and informative reading.

R. H. OPPERMANN.

TERNARY SYSTEMS, Introduction to the Theory of Three Component Systems by G. Masing. Translated by B. W. Rogers. 173 pages, drawings, 15 × 23 cms. New York, Reinhold Publishing Corporation, 1944. Price $4.50.

This is an exposition of the fundamentals underlying ternary systems, a subject of increasing importance, particularly in the alloying field. Its complexity and diagrammatic means of representation are inherently more difficult than singularly or binary types which makes it less easy to comprehend a problem. The work here covered therefore gives explanations based on formal thermodynamic laws, and the use of the phase rule.

At the outset the phase rule is taken up with an introduction assuming familiarity with the simple form of this law as applied to systems of one or two components. It is shown that there is the highest number of independent variables which can exist in a ternary system at constant pressure and from this fact comes the possibility of representing the states of a ternary system in a space diagram. The variables are the concentrations of two of the constituents and the temperature. The triangular method of representation is used. Consideration is given to a ternary system in which the corresponding binary systems are simple mechanical mixtures

of the components. These possess eutectics, but form neither solid solutions nor compounds. Then attention is centered on ternary systems of the unbroken solid solution type in which the surface of the beginning of crystallization exhibits a maximum or a minimum; system having a binary compound with an open maximum but without formation of solid solution; solid solutions with solubility brakes in two binary systems. At this point consideration is given to what may be viewed a more general form of the problem of mechanical mixtures previously treated on—a ternary system for which all three binary systems exhibit eutectics and possess limited solid solubility; and, likewise, another thread is picked up later in the topic of systems with solubility gaps in the liquid state where, in important metal systems, the constituents are soluble in each other in the liquid state to only a limited degree. Examples are the lead-containing copper alloys and the systems lead-zinc, lead-aluminum etc.

The latter portion of the book is devoted to topics of two binary systems from unbroken series of solid solutions, the third has a gap in miscibility in the solid state; iron-silicon-aluminum alloys; tin-zinc-copper alloys; the allotropic forms of iron in the ternary system. There is a bibliography and a subject index in the back of the book.

The presentation is made on the basis of metallic alloys. The conclusions are valid also for non-metallic systems as long as the vapor pressure is negligible. The book is a means to an understanding of the cases which occur in practice, and it is a foundation for the working out of more complicated systems.

R. H. OPPERMANN.

RADIO DIRECTION FINDERS, by Donald S. Bond. 287 pages, illustrations, 14 × 22 cms. New York, McGraw-Hill Book Company, Inc., 1944.

This subject belongs to the class which has grown to the proportions of a specialty. No longer is it possible for one with just a passing interest to keep abreast of developments. And, too, the demand for those with understanding of the specialty parallels its growth. This book is a text for electrical engineers who desire to specialize in direction finders. Written on a graduate level, it gives qualitative descriptions of practical systems in use with an analytical study of underlying phenomena. General considerations are first taken up, including standard test procedures and other material of a reference type. Wave propagation is next taken up, followed by chapters on directive antenna systems, Aural-Null direction finders, performance characteristic of loop input circuits, visual direction finders, and radio navigation aids. Appendixes give radiation due to an infinitesimal dipole, calculation of field strength for propagation over plane earth, extended dipole antenna and phase relations in coupled circuits. At the end are subject and author indexes. The work is a good, thorough coverage using mathematics to best advantage in its method.

R. H. OPPERMANN.

NEW ARCHITECTURE AND CITY PLANNING, edited by Paul Zucker. 694 pages, photographs, 16 × 24 cms. New York, Philosophical Library, 1944. Price $10.00.

This symposium is the work of many collaborators who base their presentations on a practical background which is in contrast to the popular visions of dreamers. Architects and engineers, city planners and administrators, sociologists and scientists, all have contributed. The work is divided into six parts, each consisting of papers by the various contributors as subdivisions. The first is devoted to specific building types and covers industrial architecture, department stores, office buildings, the house, theaters, etc. New materials and new construction methods is the heading of the second part. Here can be found the effect of science on the home, prefabrication methods, and such materials as plywood, plastics and glass. The matter of housing, including the problems of replacement of existing low-cost houses, comprises the subject matter of the third part. Parts four and five deal with city and regional planning, and the problem of a new monumentality. The latter is novel in the usual architectural discussion. It is extremely interesting and instructive. The last part of the work is devoted to education which touches on citizens' interest in planning; the situation of engineers, architects, and sociologists; education of the future architect, etc.

A great many ideas and visions are presented, which, when observed as a whole, may point to a common comprehensive direction. Those whose interests are first rank as well as others who have general interest may obtain much from the many problems brought forth and their solution in this book.

R. H. OPPERMANN.

HISTORY OF COLOR PHOTOGRAPHY, by Joseph S. Friedman. 514 pages, illustrations, 24 × 16 cms. Boston, The American Photographic Publishing Co., 1944. Price $10.00.

The history and development of color photography prior to 1925 has been covered by E. J. Wall in his work "The History of Three-Color Photography." Since then, progress has been rapid. Viewpoints and emphasis have changed so much that present practice uses techniques that received hardly any mention in 1925. Color photography passed from the use of the cumbersome one-shot camera with its delicate light splitting device, to the monopack which could be exposed in an ordinary camera. It discarded the complicated positive processes which required precise registration of three separate images, and adopted the multi-layered material processed by color development or by silver-dye-bleach. The book at hand is of similar nature as the 1925 work, but covers progress to date.

It starts with the earliest ideas of colorimetry, the science that deals with the specification and measurement of color, and it points out that any color can be matched by a suitable mixture of three basic colors. This thread is followed in the treatment of subjective color reproduction and objective color reproduction which lay the foundation for the discussion on color cameras. Here the five classes of light splitting devices are taken up. They are two or more lenses without beam division, and behind inclined glass plates both of which are of academic interest, one lens in front of beam splitters, two or more lenses behind beam splitters, and a divergent lens in front of two or more lenses. This early part of the book gives a good illustration of the method of presentation throughout. The topic is first chosen, descriptive matter and theory is given, then development is recorded with special attention to U. S. and foreign patents.

Next discussed are the photographic emulsion, tripacks and bipacks, monopacks, Kodachrome and Kodacolor, and Ansco Color and Agfacolor. In the treatment on screen plates there are outlined eight general classes with which the techniques used for the making of the screen could be grouped, and later under the heading "Formation of the Screen" the specific methods contained in each of the groups are given space. The first discussed are those procedures that do not yield geometrically patterned screens, of which the most important are the ones utilizing "dusting-on" schemes. Later the coverage goes into the Lenticular Process, Masking, Toning, Color Development, and other processes. At the end there are chapters on diazo and bleach-out photography. It is pointed out that no great strides have been made in these fields to warrant the hope that they may some day offer a solution to the print problem, but they do suggest a possible extension of useful light-sensitive systems. At the end there is a list of periodicals and a subject-author index.

The book represents a tremendous amount of work in the search and digestion of widespread literature, the logical presentation of subject matter, expert knowledge, and a great amount of experience. It is a valuable tool, particularly for further research.

R. H. OPPERMANN.

PUBLICATIONS RECEIVED.

The Art of Resuscitation, by Paluel J. Flagg. 453 pages, illustrations, 15 × 23 cms. Reinhold Publishing Corporation, 1944. Price $5.00.

The Theory of Resonance and Its Application to Organic Chemistry, by George Willard Wheland. 316 pages, tables and illustrations, 14 × 22 cms. New York, John Wiley and Sons, Inc.; London, Chapman and Hall, Limited, 1944. Price $4.50.

Aerodynamics, by Leslie R. Parkinson. 112 pages, illustrations, 15 × 22 cms. New York, The Macmillan Company, 1944. Price $2.25.

College Physics, by C. E. Mendenhall, A. S. Eve, D. A. Keys, and R. M. Sutton. 693 pages, illustrations, 16 × 24 cms. Boston, D. C. Heath and Company, 1944. Price $4.00.

Outline of the Amino Acids and Proteins, edited by Melville Sahyun. 251 pages, tables and illustrations, 16 × 24 cms. New York, Reinhold Publishing Corporation, 1944. Price $4.00.

Fats and Oils, An Outline of their Chemistry and Technology by H. G. Kirschenbauer. 154 pages, illustrations, 15 × 24 cms. New York, Reinhold Publishing Corporation, 1944. Price $2.75.

The American Annual of Photography 1945, Volume 59, edited by F. R. Fraprie and Franklin I. Jordan. 200 pages, illustrations, 19 × 25 cms. Boston, American Photographical Publishing Co.; London, Chapman and Hall, Ltd., 1944. Price (cloth) $2.25, (paper) $1.50.

CURRENT TOPICS.

ARMY AND NAVY NOTES.

B-29's Nose Section Is Air Conditioned Cabin.—(*Refrigerating Engineering*, Vol. 48, No. 3.) How the nose section of the new Boeing B-29 Super-fortress is constructed like a gigantic air conditioned cabin for the comfort and combat efficiency of its crew members was described recently by technicians of the De Soto Division of Chrysler Corporation.

The nose section and certain other portions of the plane's fuselage are protected against cold, heat and the atmospheric pressure of high altitudes. This means that the B-29 is the first combat bomber in which a comfortable air pressure condition can be maintained over long periods of time, while the plane flies at high stratosphere altitudes.

To achieve this comfortable air conditioned effect, two things are necessary: The cabin must be sealed so no air can get in or out of it except by pumps; and there must be a series of pumps which keep the air pressure and temperature inside the cabin at a constant level even though the ship may rise into high altitudes where the temperature and pressure are very low.

Before the B-29, crew members of planes capable of flying the stratosphere were limited in the altitudes in which they could fly and even at 20,000 ft. had to wear oxygen masks and cumbersome flying clothes. The wearing of oxygen masks for long flights, however, often resulted in fatigue marked by dullness and inert reactions.

In the B-29, however, the plane can fly at extremely high altitudes for hundreds of miles with the crew thoroughly protected in pressurized compartments. Once in the combat area if the ship comes down to lower levels the cabin can be depressurized.

The job of sealing the nose section calls for an intricate and complex treatment whereby every seam, joint and every one of the thousands of rivets must be sealed by gummed tapes, putties and sealing compounds. Even repair rivets must be dipped in sealing compound and dried before installation in the unit. This is entirely a hand operation requiring many man-hours of labor, since the nose section comprises more than 16 per cent of the entire craft. The efficiency of the seal has been proved in combat, but Chrysler engineers are at work on still better new sealing materials and new methods which will increase both the efficiency of the seal and the speed of manufacture of the units.

R. H. O.

Coal Needed for Each Ton Used in a Boiler Plant.—T. W. REYNOLDS. (*Heating and Ventilating*, Vol. 41, No. 8.) It is hard to conceive of anything we do that does not use fuel directly or indirectly. Production, communication, transportation, all require fuel. While the problem of tracing the tons of coal, equipment and material required to deliver one ton of coal to the boilers is endless, there are certain places where the coal usages incident to

that one ton of coal consumption are more readily known. They are the railroads and the mines.

In delivering this one ton of coal the railroads will consume 0.25 ton in hauling and switching and in the pro-rated usage including that at engine houses and shops. The one ton will travel 360 miles, the average haul by rail. Fifty tons will fill one hopper car with either bituminous or anthracite, and over 20 days are required per round trip. Ever increasing transportation difficulties are increasing this turn-around time and with it the size of the stock pile in transit at the expense of stock on hand. Thus, each ton of coal saved will require less of many things, such as weight to haul, coal cars, locomotives, train miles and crews, wear and tear on railroad facilities, and less interference with war transportation by slower moving coal trains.

It took some coal miner about one-third of a day to produce the $1\frac{1}{4}$ tons of coal at the mine. It also took mine timber and cut lumber, steel, copper, machinery, and operating and maintenance supplies at the mines.

At the retail yard, delivery included, one ton of coal saved per day would in one week save one man day, an average of 20 truck miles and 80 gallons of gasoline, plus wear and tear on truck and tires.

Now back to the boiler room where that ton of coal is consumed, wisely or unwisely. Here we find a lot of boiler plant auxiliaries. These are, in the main, electrically operated. All take something that came from the burning of fuel. Such equipment is more numerous than we realize until we take inventory. There are fans for forced and induced draft and ventilation, pumps for boiler feed, cooling water, condensate, oil and feed water, coal crushers, automatic coal scales, stokers, coal handling, and ash handling systems. Steam is used for soot blowing. This is a major though partial list.

Now the fuel used for all this is not large relatively, only about one per cent, and this could easily be surpassed by lack of efficient boiler operation. In many plants an item of one per cent means many tons of coal per day. This shows the added importance for proper maintenance and operation of the auxiliaries.

R. H. O.

By-Product Chains.—By-products of by-products of the by-products of farm crops have proved of value in at least two instances in the U. S. Department of Agriculture research program on plastics from farm wastes. The Northern Regional Research Laboratory at Peoria, Ill., has recently reported on these materials three times removed from the crops. Successive industrial processes use part and leave part. The part left in each case turned out to be of further use when the laboratory men had found the use and also had found out how the leftover could be worked into still another product.

With corn the main crop, the laboratory has developed many uses of the grain—including "steep liquor" in manufacture of penicillin. But the cob was a by-product. Since the war started the experimenters have ground the cob and sifted out the harder parts as a valuable by-product. This horny fraction is used in an air blast for cleaning machined metal parts. The air-driven corncob grits will take off grease and loose bits of metal, but they are not hard enough to scratch or grind down the machined metal. The finer

and softer parts of the cob are sifted out, and for some time seemed waste—perhaps of slight value as fertilizer.

The cob flour closely resembled wood flour. As the name indicates, wood flour is finely ground wood. It is a raw material for plastics. Combined with phenol-formaldehyde resins, wood flour forms plastics. Substituting cob flour produced an inferior plastic. But the experimenters persisted and by modifying the binder resin to fit the cob flour they got an excellent plastic, superior in some respects to wood flour plastics. When the cob flour was shifted from a waste to a by-product, this was the end of the chain of processes, because there was no waste to work with any longer.

An almost identical series of developments stems from the peanut crop. Agricultural scientists have worked with the oils and proteins of peanuts. Peanut hulls have been a primary waste. Ground peanut hulls, however, provide the basis for Noreseal, a substitute for cork used in crown bottle caps. A fine hull flour remains, and a suitable binder has been developed to transform it into a plastic similar to those from wood or cob flours. All parts of the peanut can be used up in productive industrial uses.

R. H. O.

Adjusting the Frequency of Quartz Oscillator Plates.—Equipment for precisely adjusting the frequency of quartz oscillator plates downward was announced recently by engineers of North American Philips Company, Inc.

This special X-ray apparatus, designed by Philips, employs a new high-capacity water-cooled tube that attains the highest possible efficiency. Because the apparatus utilizes an extremely intense beam of X-rays, the problem of protecting the operator from stray radiation was of paramount importance. This difficulty was overcome by using a rotary fixture that exposes one crystal to the beam while another crystal is being loaded into a second holder.

The holder in front of the X-ray window is connected, by means of its electrodes and a system of contacts, to a pair of insulated external leads—these may be connected to an oscillator. In this manner the downward drift of the frequency can be accurately measured; thus, the crystal may be removed from the beam when the necessary change has been accomplished.

The crystal connection arrangement contributes much to speed and convenience in production work. The front electrode is open, exposing a definite percentage of crystal area to the X-ray beam. The most efficient area size to use was determined through experimentation.

Depending on the original characteristics of the plate, frequency may be lowered in the X-ray unit at a rate of 30 to 50 cycles per second per minute. It is possible to change the frequency of plates in the 6–8 megacycle per second range from 2–3 kilocycles per second total—this is the saturation value of such crystals. Higher frequency plates can be changed over a larger range.

The crystal holder is arranged so all crystal sizes from .4″ x .4″ up to .75″ x .75″ can be accommodated. Two crystals of each size may be placed in each holder—one is rotated to the X-ray beam while the other is being loaded. In this manner, oscillator plates may be placed successively into the beam giving complete protection to the operator. There are two crystal holders, one for each window of the X-ray tube.

North American Philips Company developed the X-ray equipment for irradiating crystals at the request of the U. S. Army Signal Corps. Dr. Clifford Frondel, of Reeves Sound Laboratories, has experimented with this method which requires no artificial loading of the oscillator plate with foreign material. Not only that, the method permits exercising minute control over the process.

Dr. Frondel's experiments show that the change in frequency is permanent throughout and beyond any temperature range that the crystal is apt to experience. Actual factory applications of the technique include:

1. Recovery of over-shot crystals which have been carried too far in finishing.
2. Precise adjustment of standard crystals for use in calibration and in testing.
3. Manufacture of precision crystals for frequency and time standards. A crystal which is considered stable can be precisely adjusted to final frequency by this method without the possibility of further aging.
4. Adjustment of frequency of a standard crystal at a given temperature.

The X-ray unit for crystal irradiation utilizes highly efficient energizing equipment with a rated output of 60 KV at 25 MA. It is a full wave assembly, utilizing two rectifier tubes. The water-cooled X-ray tube insures continuous operation at peak electrical output. Controls are conveniently located.

R. H. O.

Electronic Device Measures Muzzle Velocity of Projectiles.—American Army and Navy guns of all sizes are blasting the enemy on every war front with more deadly effectiveness than ever before because of amazingly accurate muzzle velocity measurements, as precise as 1/100,000 of a second, made possible by a new electronic time-interval counter developed in RCA Laboratories at Princeton, N. J.

Revealing some details of the device, E. W. Engstrom, Research Director of RCA Laboratories, said the instrument has been in use for more than a year at the Aberdeen Proving Ground in Maryland and at other Government arsenals and proving grounds throughout the country. It supplies instantaneously information upon which the performance of a given gun is established and the uniformity of its ammunition checked within a few seconds. It is equally effective, Mr. Engstrom said, with all types of guns—from small hand weapons to the Army's and Navy's most powerful 16-inch rifles.

Developed especially for the Aberdeen Proving Ground by RCA, the electronic time-interval counter is designed to measure, with great accuracy, a time interval in the order of one one-hundredth of a second. Extreme accuracy is obtained through the ability of the device to give this measurement to within a hundred-thousandth of a second, according to Engstrom. The research on this device was brought to fruition, and a very practical device made available to the military services through the work of Igor E. Grosdoff, RCA research engineer.

"On the ranges at Aberdeen and at other arsenals, means are being provided for making these measurements at the rate of hundreds of observations a day," Engstrom said. "Each range is equipped with two electrical coils,

arranged so that a projectile will pass through them in succession. By magnetizing the projectile, a small electrical signal is generated by each coil as the bullet passes through. If the coils are 30 feet apart, and the time between the two signals is one one-hundredth of a second, the bullet is traveling 3,000 feet a second."

The counter consists of three essential parts: an oscillator, a gate, and the counter proper, Engstrom explained. The oscillator is regulated, as in a radio transmitter, by a vibrating quartz crystal and delivers precisely 100,000 pulses each second. The gate, actually a vacuum tube circuit, passes these pulses into the counter, which counts them and finally, when the gate is closed, shows by indicator lamps the number of pulses that have passed through. In other words, Engstrom pointed out, it shows the number of hundred-thousandths of a second from the time the gate opened until it closed.

"This extraordinary speed and accuracy of operation," he continued, "is possible because there are no moving parts; the entire apparatus is electronic and can be started and stopped instantaneously."

In operation on the firing ranges, the counter's gate is opened by the electrical signal from the first coil as the bullet passes through it, and is closed again by the impulse from the second coil. The operator records the time of flight between coils and computes the velocity. It is noted down along with the record of the particular gun and projectile being tested for subsequent analysis by ballistic experts. The operator then touches the reset button and is ready for the next shot, all in a matter of a few seconds.

"The importance of such fine measurements become apparent," Engstrom explained, "when it is remembered that if all shells from a gun leave the muzzle with the same velocity, then they will all fall in the same spot, and the effectiveness of the fire will depend only on the skill and aim of the gunner. To insure this consistent performance, our arsenals are constantly measuring muzzle velocities of all types of guns and with all kinds of powder loads and shells."

R. H. O.

The Odograph.—This device is a means of instantaneous map-making on the field, and therefore a major wartime contribution. Manufactured by the Monroe Calculating Machine Company, its description has only recently been released by the Engineer Board, U. S. Army. The instrument's odd name was derived from two Greek words, "hodus"—meaning the way to, and "graph"—meaning to write. It draws a map that everyone can understand while the vehicle in which it is situated is in motion.

The Odograph is a typical example of America's wartime research coöperation. Beginning about two years ago, it was developed by the Engineer Board in a coöperative research project with the Carnegie Institution of Washington's Department of Terrestrial Magnetism, and the National Defense Research Committee.

In addition to its use for map-making, it can plot the position of objectives relative to a given base, such as the location of guns to field headquarters. It can also be used for land navigation, locating a rendezvous for combat and supply units; field artillery surveys; engineer reconnaissance and the tactical control of movements.

There are three principal units to the instrument—an electronic magnetic compass, a power pack, and a plotting disk on which a movable pencil actually draws the maps.

In order to plot a course it is necessary to know both the distance and the direction of travel. The Monroe-built Odograph determines the distance travelled by means of a speedometer attached by a flexible cable to the jeep's or truck's transmission. Direction is determined by means of a magnetic compass, properly corrected by a special Monroe-built electronic unit so that the iron and steel in the vehicle will not affect the delicate compass. These two factors, distance and direction, are fed automatically into the mechanical brain of the plotting unit. Here the mechanical brain, made of wheels, wires, cogs and a maze of intricate mechanisms, actuates the plotting pencil which draws a map of the route travelled on a sheet of graph paper placed on the map table.

The electrical power needed to operate the Odograph comes from the battery in the jeep or truck, and the special power pack raises the battery voltage to values needed in the electrical circuits.

The Odograph can plot to any scale from 1 to 20,000 to 1 to 500,000, making it possible to draw a route map, showing all the roads in a specified area to the same scale as a topographic map (which shows no roads). By placing the Odograph map drawing over the topographic map, field commanders can get a complete road and topographic map, which is just what they need to carry out operations against the enemy.

In addition to map drawing equipment the Monroe-built Odograph has an azimuth "dial" which indicates the direction of the vehicle at any moment. Special counters tell at a glance the number of miles travelled north, south, east, and west, as well as the total miles travelled. Thus a trip of eleven miles might be indicated on the Odograph as six miles north, two miles west, one mile south, and two miles east.

R. H. O.

Novel Three Element Drive Reduces Prime Mover Speeds Electrically.—(*Industry and Power*, Vol. 47, No. 3.) A unique electric device for the reduction of speed, where gear and coupling are ordinarily necessary, has been announced by the Ridgway (Pa.) Division of Elliott Company. The drive, which is the outcome of several years' research work by Thomas D. Bowes, M.E., naval architect and marine engineer of Philadelphia, will bear his name. Built in reversing and non-reversing types, its immediate use is expected to be for ship propulsion.

In addition to maintaining a substantially constant speed ratio between driving and driven members of ship propulsion equipment the new unit provides some power for auxiliary service loads through slip ring take-off circuits. Moreover it can be used as a source of electrical power for cargo handling and similar functions when disengaged from its primary job of speed reduction and torque multiplication of propulsion equipment.

Three main elements, concentrically mounted, comprise the drive. These are an engine element which is bolted to the prime mover shaft, a shaft element which is coupled to the driven shaft, and a stationary element.

Essentially a synchronous generator rotor with poles selected for the desired speed ratio, the engine element is permanently attached to the shaft of the prime mover and revolves at normal engine speed.

The shaft element consists of a rotating spider on which two armatures are concentrically mounted in relation to the engine element. The inner armature and the engine element form a generator. The inner armature winding not only generates electrical energy but also transmits torque to the driven shaft. The outer armature makes use of the field of the third or stationary element to operate as a motor, converting electrical energy into mechanical energy for turning the driven shaft.

If the shaft element is held stationary, the inner parts of the drive behave like an ordinary synchronous generator and electrical energy can be taken continuously from the drive. In normal operation, of course, the shaft element rotates at the speed desired. The relative speed between the engine field element and the shaft element results in generation of electrical power. The reaction torque, which in a normal generator would be absorbed by the frame supports, is available to turn the shaft element.

R. H. O.

South Pacific Garden is Weedless.—Soldiers and sailors, who get plenty of solid rib-sticking food in their rations, crave fresh vegetables after living on dehydrated and canned foods, according to an article in the *Extension Service Review*, published by the U. S. Department of Agriculture.

To meet this situation the Foreign Economic Administration has made H. H. Warner—on leave as Director of Extension in Hawaii—chief of its Forward Area District, and has given him the job of growing a wide variety of American vegetables in the Pacific area for use fresh in that area. Warner's experience in Hawaii gave him a practical approach and experience in dealing with conditions he would find in the South Pacific Islands.

South Pacific gardening is on a large scale. Crops include corn, cucumbers, tomatoes, watermelons, cantaloups, Chinese cabbage, radishes, peppers, and eggplant.

In a report to Extension headquarters in Washington, Warner tells of employment of native labor, the aid given by members of the fighting forces, the tractors and implements made available. Some of the planted fields have been close to fighting areas and "occasionally Jap patrols have to be flushed out of some cornfields." Plantings are under way on many islands—all unnamed in Warner's letter. Of one island he writes:

"This island is unique in that there are vast level plains covered shoulder-high with kangaroo grass. We are farming this land which runs right up to the jungle, and turning over the soil for the first time in history. It is rich black soil, entirely devoid of weed seeds so that cultivation has been no problem yet. The grass is easily killed with one plowing and disking. It is far different from the usual conception of farming on cleared jungle areas. To date we have produced good crops and have not found it necessary to use fertilizer. We have no corn earworms, very few aphids, no melon flies, but plenty of chewing insects."

R. H. O.

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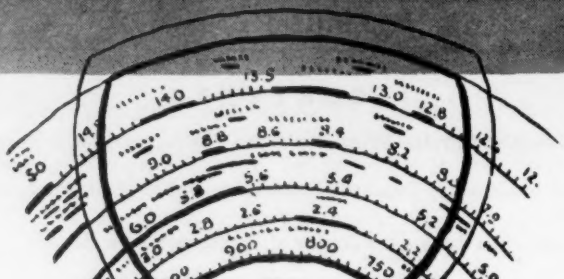

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